

Not Delivering the Goods

THE collapse of Rolls-Royce Limited last week is the most spectacular example for some time of the way in which modern technology fails to live up to the promises made on its behalf. Unhappily, this sorry business is by no means an isolated illustration of how mismanagement can make bright prospects sour. In Britain, for example, there is the Dungeness B reactor on the English Channel to warn passing ships (or at least those that without much benefit from modern technology avoid collision) that technology is as often a broken wheel as a juggernaut. When will the power station ever produce its intended supply of electricity, or even a fraction of it? But the Central Electricity Generating Board is also the inglorious possessor of a series of 500 MW generating sets so unreliable that last year 22 per cent of the country's generating capacity was out of action at any time. British Rail possesses hovercraft which are pale if noisy shadows of the prospectuses offered on behalf of these machines several years ago. The Concorde is similarly in limbo between success and failure and, even at this late stage in its development, roulette players are as competent as technical people in guessing whether more than £500 million of development money will be justified.

Not only government expenditure on technology is frustrated, however. Britain is by now well provided with electronic computers working far below capacity for lack of proper programming. Even in the United States, similar tales abound, even if there is a tendency for Americans to cut their losses sooner than other people. What, for example, has happened to the USNS Savannah? What about the turbo-electric locomotives that appeared briefly two years ago on the railway line between Boston and New York? For every recognizable and beneficent achievement there seems a minor disaster to complain of. Those who take the view (see page 459) that technology needs watching usually base their argument on the fear that too much success may rid the world of valuable features, but it is much more to the point to ask how much benefit is being lost to the modern world because of mismanaged failures.

By now, the reasons why Rolls-Royce failed are clear enough. The company argued, four years ago, that only a substantial stake in the American aerospace industry would allow it to maintain its distinguished status in the design and manufacture of aircraft engines. That done, it sought the only substantial line of business then available—the opportunity to build engines for the Lockheed TriStar passenger aircraft. Like a man convinced that without water he will die, Rolls-Royce bid for the contract not merely with zeal but with a sense that its survival was at stake. Even with good management, the result would probably have been as humiliating as the events of a week ago. Inevitably, Rolls-Royce's lowest bid was too low to make a profit. Inevitably, Lockheed, its customer, with the same sense of danger in its head, was out to get the lowest price for the TriStar engines and the kind of insurance that stiff penalty clauses will provide. The lesson of the past few weeks is not that Rolls-Royce should have

made a more realistic bid for the contract to develop the TriStar engine but that in 1966 it was in no position to bid safely. The company's fatal mistake was its assumption—much encouraged by the government of the day and even echoed by the would-be tougher fellows now in office—that a company which happened to make a reputation for itself building aero engines in the forties and fifties was destined to build bigger and better aero engines for the rest of time. The company was trapped in too narrow a market. It would have been much better for everybody, Lockheed included, if Rolls-Royce had swallowed its pride and put its technical prowess into typewriters or sewing machines.

Why did these disasters happen? To what extent is the company itself to blame? And how far are the technical people to be blamed? There is an element of technological madness in the way in which the Rolls-Royce tender for the Lockheed contract was in part dependent on the gamble that it would be possible to fashion turbine blades from carbon fibre. To be sure, the development of alternative blades from titanium has also gone ahead, but may not the daring with which Rolls-Royce proposed to move ahead in a new field of materials science have been interpreted by potential partners as a sign of solid confidence, not of gambler's nerve? After all, the engine was to deliver twice as much thrust as any other engine manufactured by Rolls-Royce, and was to have been developed in half the time. For most aero engine manufacturers, this would have been enough of an innovation.

What can have possessed the company so seriously to prejudice its chances of success by adding another inno-

Postal Strike

THIS issue of *Nature* will be the first to be distributed outside North America and parts of the United Kingdom since the beginning of the British postal strike on January 20. Missing copies will be despatched as soon as the strike is over but there will unfortunately be some delay.

Until further notice, and certainly for some days after the strike is over, contributors and subscribers outside Britain should write only to the North American office of *Nature*, 711 National Press Building, Washington, DC 20004 (telephone 202 737 2355). Communications by telex may be sent either to Washington (64280) or to London (262024).

Manuscripts for publication which may eventually be accepted but which are at present embedded in the British postal system will not have been acknowledged as received, and authors wishing to ensure safe delivery should send a duplicate (so marked) to Washington.

Potential contributors may wish to know that the incoming flow of manuscripts for *Nature* has been so reduced by the strike that the time spent on handling and assessment is much reduced. In the physical sciences in particular, publication is now extremely rapid.



vation to the list of developments in hand? The answer is that modern engineers, in government service or in industry, are inclined to exaggerate the benefits of the innovations with which they are concerned. Moreover, there is a temptation among practising engineers to suppose that only innovation can be considered respectable, with the result that the humdrum business of making sure that existing products are manufactured so efficiently that they remain competitive tends to be neglected. This, certainly, accounts for the way in which the designers of the new British electricity generating sets, as well as the technical people who put them on order, let their expectations get the better of their judgments. This is also the spirit in which the hovercraft builders put their machines in service without knowing whether they would function properly. This is also the spirit in which gullible customers have been propositioned on behalf of the computer builders. The machine is often supposed to have magic at its command. In the aftermath of the disaster at Rolls-Royce, there is an urgent need to discover the causes and the remedies for what seems to be the overoptimism of high technology.

In Britain, there are special dangers. The market is meagre. Cash for expensive development work is hard to find. People are scarce, traditions are often inappropriate. The result is that many important technical developments are sustained by inadequate resources for development and by incomplete or even incompetent assessments of the real need of them. In many ways this is the besetting sin of British industry, happy-go-lucky but well blessed with the resourcefulness that comes from pulling chestnuts out of the fire. The trouble is, of course, that the more complicated projects are those which cannot safely be left to chance. The armies of development engineers dancing attendance on innovations such as military rockets or computers in the factories of the United States may be larger than is strictly necessary, but their presence is often a salutary reminder that competitors elsewhere are inclined to fancy their chances not as equals but as little heroic Davids. The fallacy is that even if the old biblical story were true, the chances that the underdogs will come out on top in modern technology are even smaller than the chances that the favourites will lose in horse racing. The rewards of success may be high but so must be the stakes, which in technology implies that the cost of development is usually much higher than people, companies and governments like to contemplate. The moral for countries like Britain, however, is sad but plain. In times when the development costs of radically new types of aircraft are of the order of £500 million, when it costs as much to bring a new type of nuclear reactor into service, when even aircraft engines may cost £100 million or more to develop, when computers may be even more expensive, radically new developments are likely to be beyond the resources of individual companies and, if governments are for political reasons unwilling to foot these bills, countries must simply forgo the sense of pleasure which advanced technology provides and must instead limit their ambitions to schemes they can afford.

There are also lessons to be learned in the organization of technology and the science from which it stems. Practitioners are frequently tempted to suppose that the possible must necessarily be feasible. In the best traditions of George Stephenson and his successors, engineers tend to suppose that it is only necessary to conceive of an

innovation to lay the groundwork for its realization. And, of course, part of the excitement of technology is that this principle is often realized in practice. In this sense, the Manhattan Project during the Second World War has cast a long shadow over the decades which have followed, for it has seemed to many people in technology to be an illustration that it is only necessary to devote enough people and resources to a technical project to make it work and even sell. Even if this belief were always upheld, of course, it would not follow that money spent on development of a new idea would be recompensed in the market place, where much depends not merely on technical success but on timing, competition and even luck. In the circumstances, there is an obvious need to ensure that scientists and engineers in industry are more immediately conscious that the social function of technology is not so much invention but, rather, some social benefit, often prosperity. It is probably no accident that some of the most successful industrial enterprises in Europe have done just this by making sure that design engineers are responsible at least for the development of their new ideas and sometimes even for bringing them into commercial production as well. Much can be done by making sure that engineers and scientists are educated to the need to make their craft of value, but the better organization of industrial enterprises of all kinds is obviously also an urgent need.

In circumstances like these, the British government has a difficult job to do. In the first place, it is evidently now necessary to have a much more explicit policy on the relationship between government and industry than the Conservative party has been allowed by its convictions to develop. To be sure, the government is nothing if not consistent in saying that it will not countenance attempts to bolster up industrial enterprises so as to avoid unpleasant side effects—patches of unemployment or even the loss of national prestige. In this sense, the fact that the government now proposes to nationalize some parts of the bankrupt aero engine company is strictly a red herring—any government would have been compelled to do as much for such an important contributor to national defence. The Conservative government's weakness in its policy on industry is, unhappily, the way in which its declared policy of letting industry stand on its own feet has led it to turn lukewarm towards public functions which only governments can adequately perform. It may be true that government laboratories and government grants have in the past provided industrial enterprises with too much assistance or at least advice, with the result that there has been no clear division of responsibility between government and industry. The danger now is that the government may abandon long-term projects which contain the seeds of future industries in the belief that these are matters which commercial people should look after for themselves. After all, one immediate effect of the collapse of Rolls-Royce will be to make all kinds of other British industries look critically at their commitments to technical innovation and there will be a danger that many potentially rewarding jobs will be abandoned. If the result were a concentration of the available resources on the development of products likely to be competitive with those of industries elsewhere, everybody would be glad, but the more likely outcome of the present heart searching in British industry will be that resources for technical development are still further under-used.

More Room on the Moon

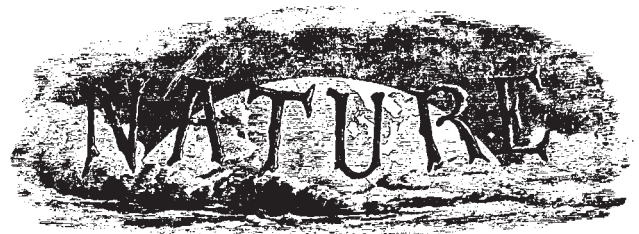
THE visit to the Moon last weekend seems to have demonstrated how quickly Moon landings have become familiar and even dull affairs. Even with the help of colour television, and in spite of the skill with which the television networks now switch their shots from one place to another, only the more dramatic phases of a Moon journey seem to be able to compete successfully with the well-established peak hour shows. To be sure, it would be different if there were some kind of accident; then the public attention for the Apollo programme would be as avid as ever—the flight of Apollo 13 showed that for all to see. But left to itself the international audience for these journeys will tail away as the months go by, just as the Apollo programme itself has been attenuated. All this is sensible enough and, indeed, it may well be that inattention from the public is what space exploration in the United States and the Soviet Union is most in need of. It does not unfortunately follow that everything is right.

In the United States, the most serious defect of the programme now being written into the budget is that too many efforts are being concentrated on the still pointless task of developing large habitable satellites together with the re-usable rockets which can carry people in both directions. It is true that final decisions on the so-called shuttle have not yet been taken—so far, substantial sums are being spent only on the development of the engines for the shuttle spacecraft, and engines are inevitably the slowest items to develop. But the more money that is spent, the harder it will be for NASA to abandon what has been done already. As time goes on, however, the arguments in favour of the plan to build a shuttle and to launch space stations for it to service remain embarrassingly short of justification. The argument that the shuttle and its space stations are necessary steps in the development of hypersonic flight is of course entirely an illusion. That is a project which should and could be pursued in its own right. The argument that there may be opportunities for using orbiting space stations as places in which to carry out the kinds of experiments in such techniques as welding which the Apollo 14 crew attempted last weekend is a pitifully thin excuse for spending such a lot of money. Does anybody seriously believe that there will be a commercial industry based on such delicate capering? And the belief that the space stations and their transport vehicles will somehow be justified by the presence of sufficiently large telescopes is tenable only in circumstances in which astronomers are not in the majority. Since it appears that the first task of the first orbiting space station will be to carry out some no doubt valuable astronomy, particularly in the X-ray region of the spectrum, it might be interesting—it would certainly be equitable—if NASA were to ask a representative body of astronomers how they would like to spend the sum of money involved—\$15,000 million is probably at this stage a modest estimate.

The starting point for the shuttle concept seems to have been the recognition that only a very large project could take the place of the Apollo programme, due originally to have been completed by the mid-seventies. Even with the experience of three successful landings on the Moon, it now seems clear that human beings are not particularly successful at lunar locomotion. Understandably, they find it hard to walk uphill. Understandably, their still

primitive suits leave much to be desired. Understandably, in circumstances in which it is necessary to carry all kinds of physiological equipment for the maintenance of life along with the people, it is hard to provide sufficiently complicated and comprehensive instruments to return a rich harvest of information. This is why many Americans must be regretting the abandonment of the Surveyor programme, with its promise of being able at least to fill in the gaps left by the necessary sporadic visits of human beings and possibly able to extend enormously the work (not to be sniffed at) already carried out on the Moon. But if all this is apparent where the Moon is concerned, at its comparatively modest distance, how much more so is it necessary that the long-term exploration of the solar system should be based on automatic rockets of one kind or another? As it happens, this is what the Russians have always said and it is surprising that the planners in Washington have so consistently been able to turn a deaf ear to such declarations. It will be a great misfortune if in the coming months, when the Senate is picking over the NASA budget, some effort is not made to correct a modest but potentially crucial deficiency in the present pattern of expenditure. For the rest, and because the notion that there should be large numbers of astronauts in service has become almost a historical fact, it will be necessary to decide how best to use the manpower that exists or which threatens to exist, and from this point of view there is in practice a strong case for arguing that they should be used for as diverse a set of roles as possible, if only to make sure that their employment will at least provide a clear picture of the circumstances in which men may and may not be useful.

100 Years Ago



PHILADELPHIA

Academy of Natural Sciences, Nov. 8.—Mr. Thomas Meehan referred to a potato presented to the Academy some months ago by Mr. Henszey, a member, which had the appearance of one potato growing out of the centre of another. The opinion of all who saw it was that it was really a case of this kind. It had been handed to him by the curators, and on dissection, though no exact place of origin could be traced, there seemed nothing to indicate any other theory of origin than that one potato had really grown out of the centre of the other. But there were serious physiological reasons in the way of such a theory. A potato tuber is really but a thickened axis, in which the greater part of the interior structure would be incapable of developing a bud which would produce a tuber such as this one had done. The origin of a new tuber from an old one would be nearer the old one's surface. He had been looking for some further explanatory facts, and believed he had them here this evening, in the potato tubers he now handed to the members. They were about the size of hen's eggs, and were pierced in every direction by stolons of the common couch grass, *Triticum repens*. They had gone completely through, as if they were so much wire, and in one instance two tubers had become strung together by the same stolon, as if they were two beads on a string.

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OLD WORLD

SPACE POLICY

Black Arrow under Fire

THE demise of Rolls-Royce last week has left the Black Arrow project with engines for only the next two launches. But, according to Mr J. Lewis, of the R&D Branch of the Ministry of Aviation Supply, the project should not be too severely affected because it has enough slack in it to survive a delay in the engine contracts of two or three months. Mr Lewis was giving evidence to Subcommittee B of the Select Committee on Science and Technology, whose members were again interviewing representatives from the Ministry of Aviation Supply about Britain's space policy, and it was clear that the engine contracts are not the committee's only cause for concern over the Black Arrow launcher.

The conversation during the meeting first concentrated on policy towards international cooperation in space research, but the best formulation of the policy of the Ministry of Aviation Supply was provided by the witnesses in their written evidence. "Unless the financial resources made available to national space activities are increased by a very considerable, and arguably disproportionate extent, there is really no option so far as major projects are concerned as between a national effort and an international one. The choices are rather in terms of what international projects should be supported, and what is the extent of the national programme required as a back-up to such projects."

When members of the select committee asked about Britain's role in INTELSAT, however, they were told that the Ministry of Posts and Telecommunications is responsible for that aspect of international cooperation, and the Ministry of Aviation Supply therefore cannot take an overall view of the situation. As far as ELDO is concerned, however, Mr A. Goodson, Head of the Space Division at the Ministry of Aviation Supply, explained that British policy is that while

launchers can be obtained cheaply from the United States, the advantages in building up a viable European launcher programme are more than offset by the costs. Although the government still retains an option to participate in the development of the Europa 3, he therefore believes that such an option will not be taken up.

Sir Harry Legge-Bourke then asked whether similar considerations apply to participation in the post-Apollo programme, and he was told by Mr Goodson that the underlying argument is broadly the same—the number of launchers required in the programme would still not be sufficient to justify the expenditure on their development, and the present thinking in the Ministry of Aviation Supply is that it would be better to buy launchers from the United States for European or UK satellites. Mr Goodson also told the subcommittee that no fresh information about the post-Apollo programme has emerged since the European Space Conference last November. The cost estimates made by NASA have decreased slightly, he said, but there is still no clear definition of the overall nature of the project.

If British space policy is geared so as not to duplicate expensive developments in the United States, Mr Ronald Brown wondered why the Ministry of Aviation Supply is developing the Black Arrow launcher instead of buying Scout launchers from the United States. But Mr Goodson replied that the Black Arrow is a research tool which will be used for launching a series of technological satellites, and that the expertise gained from both launcher development and from satellite development is likely to offset the costs of research and development. The project is, however, under review, he said, and the relative costs of developing Black Arrow as an independent launcher and buying a similar launcher from the United States will be taken into account. When asked by Mr Arthur Palmer, chairman of the subcommittee, whether the ministry would have bothered with development of Black Arrow if it had been able to foresee some of the difficulties that have arisen both on the technical and on the economic side, Mr Goodson was unable to provide a clear answer.

COMPUTERS

Picking Up the Threads

THE subcommittee of the House of Commons Select Committee on Science and Technology which is investigating the British computer industry met last week under new circumstances. There are several new faces on the committee because some members failed to be re-elected in the general election last June, or have moved on into the government, and there has been a restructuring of the government departments which

the subcommittee painstakingly investigated last year to disentangle the mechanism of support for the computer industry. With the subcommittee happily re-established, the investigation is again under way. A series of meetings with representatives from government departments and with Sir John Eden, Minister of Industry, has been lined up for the coming weeks to acquaint the subcommittee with the present arrangements for looking after computing interests. Under the circumstances, the chairman of the subcommittee, Mr Airey Neave, must be thinking that the investigation has much in common with the painting of the Forth Bridge. Last week the subcommittee received an updating of a memorandum presented by the National Computing Centre Limited a year ago.

The chairman of the centre, Sir Robert Cockburn, pointed out that the idea of the National Computing Centre as a centre of excellence in computing has now disappeared, and the present aim is to be an organization which provides services to computer users, including government departments. The representatives of the centre, which included the director, Dr A. A. Robinson, and two heads of groups, made great play of the critical position in which they find themselves. Some called it a dilemma and others likened it to tight-rope-walking: the position is essentially that a non-profit-making centre might just as well become a government department, losing its independence, whereas if the centre were to make money it would frighten the computer industry into asking for the centre to be closed down. As it is, the centre is not self-supporting, although the representatives agreed that when it was set up in 1965 the intention of the then Minister of Technology, Mr Frank Cousins, was that the centre break even. But Dr Robinson and his colleagues argued that the pioneering work which the centre carries out and the services which it provides are not compatible with the aim of self-support. To the dissatisfaction of the subcommittee, however, none of the representatives of the centre could throw any light on when the apparent change of policy since Mr Frank Cousins's assurances took place. The director, Dr Robinson, agreed that the position of the centre with regard to self-support had not been properly thought out when it was established in 1965. For the financial year 1970-71 the budget of the centre is £975,000, of which £589,000 is the grant-in-aid, and membership fees and earned income are only £116,000 and £270,000 respectively.

The centre is expecting a reduced grant-in-aid next year of £550,000, but is hoping for a larger income from membership fees of £125-£135,000 and

Editorial Cooperation

THE French scientific journal *La Recherche* and *Nature* have come to an agreement on editorial collaboration in the years ahead. In future, *Nature* will rely on the staff of *La Recherche* for a continuing record of news of scientific developments in France. *Nature* will provide *La Recherche* with information about scientific developments in Britain and abroad.

an earned income of £350,000. But Mr Ted Leadbitter of the subcommittee still felt that the financial situation of the centre is questionable and does not support the staff of 181 (of whom about 125 have "some professional standing or some experience of the computing industry"). The National Computing Centre is represented on 73 committees, and is prolific in its publications. Mr Leadbitter said that the centre must be either doing its job defectively, or with a sense of being overburdened.

FISH PATHOLOGY

New Laboratory

THE disease which has disastrously afflicted salmon in the rivers of Great Britain and Northern Ireland during the past few years is to be the chief subject of research at a laboratory newly established in Weymouth on the coast of Dorset. So far the cause of the killer disease, ulcerative dermal necrosis, which is characterized externally by skin lesions, has defied identification. No bacteriological or fungal agent has been isolated, and the search is continuing at the new fish pathology laboratory, which will also serve as a source of advice and practical assistance for other laboratories, fish farmers, river authorities and so on with diseased fish to be investigated.

Although the laboratory at Weymouth was set up in a renovated coastguard's house about two years ago, in association with the Ministry of Agriculture's salmon and freshwater fisheries laboratory in London, it is not yet fully operational. The most important set of equipment, which is not quite ready for use, is an experimental unit consisting of four rooms in which fish can be quarantined in 350 gallon tanks of chilled water (sea or fresh) to ensure that toxins and infection cannot reach them. In these conditions it will be possible to investigate the transmission of ulcerative dermal necrosis and other diseases from one fish to another. Work with this experimental system will occupy much of the attention of the laboratory's staff of fifteen, which includes four fisheries scientists. When some useful results have been obtained, the study of the transmission of ulcerative dermal necrosis will move out to sea.

The possible effects of pollution on fish disease will be another important aspect of work at the laboratory, and other preoccupations will be fish haematology, histology, bacteriology, and mycology, as well as virology, which has received little attention outside the United States. The first requirement for virological research into ulcerative dermal necrosis is to develop cultures of salmon tissue; because this disease does not occur in the United States, very little is known about tissue culture of salmon. A great deal of preliminary effort is therefore being devoted to the develop-

ment of basic techniques before investigations can be initiated and a possible viral cause of ulcerative dermal necrosis can be sought.

COMPUTERS

Language Levels

THE second of the 1971 Infotech State of the Art Lectures was held at the Café Royal, London, last week and will be repeated in Amsterdam on February 16-18. The lectures are now well established in the computing world and the theme of the latest meeting was the rather open ended topic of high level languages and their development.

Although the past twenty years or so have seen very rapid advances in computing science, developments on the hardware side have far outstripped the relatively meagre improvements in software and programming languages. The fundamental stumbling block seems to have been the lack until recently of a scientific approach to a problem which is now seen to be neither trivial nor simple to solve—that of setting up and applying proper design criteria for high level languages. A broad definition of a high level language was that the level of a language is a measure of its independence of the machine itself (for example, the user of square root facility in ALGOL is not usually conscious of the multitude of processes which the machine performs in the course of the calculation). But this definition did not meet with unanimous approval.

It is also possible to subdivide high level languages themselves into special purpose and general purpose languages. The value of special purpose languages (such as those specifically designed to be suitable for simulation problems) was discussed by Mr P. J. Kiviat (College on Simulation and Gaming, Institution of Management Sciences, US). Although he drew attention to many of their latent disadvantages, such as availability on only a few computers, limited vendor support and the uncertain future of any one language, he emphasized that the total time from the start of programming to the output of the results could be as much as twenty times less than the equivalent time using FORTRAN, for example. This advantage has, however, to be offset against the faster machine execution time with a FORTRAN program. But once the programmer can competently handle a special purpose language, it becomes a good economic proposition, albeit for the solution of a fairly narrow range of problems.

Professor E. W. Dijkstra (Technical University of Eindhoven) probed more deeply into the reasons why software development and applications have improved so little compared with hardware. He made the interesting suggestion that the wrong qualities are being encouraged

in programmers; they should break away from their image as puzzle-minded individuals looking for "clever" solutions to problems. In Dijkstra's opinion, many of the aptitude tests used to pick out good potential programmers were completely outdated and quite often the wrong type of programmer is selected. One objective should be to make programming easier and to make the detection of faults—one of the largest millstones around the programmer's neck—a much more efficient process. In the interests of significant improvements in software quality, he suggested several yardsticks for new high level languages which included the abolition of "go to" statements (which encourage excessive interrelationship between remote parts of the program); as well as encouraging "sequencing discipline", such a move would make debugging very much quicker.

The remarks by Mr A. D. Falkoff (IBM, Philadelphia) on the design of a general purpose programming language were a useful complement to Dijkstra's ideas about the needs of future languages. He considered that as well as being easy to learn and formally manipulable, they should be thought provoking and the documentation should "require neither a microscope to read it nor a truck to carry it". A rather controversial suggestion was that a programming language should play on the artistic instinct of the programmer so that he would develop an instinctive feeling for what was right in a particular situation. The obvious objections to Falkoff's proposals were made during the discussion, namely, that anything that was too thought provoking or that encouraged an aesthetic outlook might eventually be responsible for a great number of programming errors. Several speakers put forward the IBM general purpose language PL1 as an example of an effort to build up a general purpose language without sufficient regard for the work done on programming language theory. The result is a language which is still not in its final form and which one speaker unkindly described as "seven thousand afterthoughts in search of a language".

The interrelationship between machine design and programming languages was emphasized by two speakers with rather different points of view. Mr J. W. Buckle (ICL) looked at the subject through the eyes of the manufacturers and emphasized the inevitable conflict between exploiting one machine for maximum efficiency and exploiting the portability of programs and data across a range of computers (such as the 1900 series) or to alien machines. He also spoke of the increasing call for mixed language installations and programs from which users hoped to get the best of several worlds; but, he concluded, "complexity and generality go hand in hand

with inefficiency in specific areas". Professor R. S. Barton thought that the word "language" itself was misleading because a programmer does not "talk with or to a computer but through one", and therefore the architecture of the machine must be closely associated with the programming language. Although the nature of ALGOL had been a great influence on the design of several computers (for example, some of the Burroughs range), it seems that these concepts are yet to be fully exploited.

METEORITES

Crown Minerals

A BILL designed to prevent meteorites being sold to museums for scientific study at inflated prices, and also to bypass the wrangles which often develop over ownership of meteorites, has been laid before the House of Lords. Introduced by Lord Cranbrook, a trustee of the British Museum, the bill proposes that all meteorites which fall in Great Britain should automatically become crown property, in much the same way that some archaeological samples are designated as "treasure trove". What this means in practice is that the British Museum (Natural History), the National Museum of Wales or the Royal Scottish Museum will have the first rights to all meteorites which fall in Great Britain.

Ownership of meteorites in most countries is less than clear, and this has often given rise to a situation in which scientific research is hampered by the desire to establish ownership. The lucrative market which has developed among mineral traders, especially in the United States and Germany, has exacerbated the situation, and often a museum can only get hold of a sample of a meteorite if it is prepared to pay the full market rate. But perhaps the most important difficulty is that research is often delayed by these ownership wrangles.

Speedy recovery and analysis of meteorite samples is particularly important for cosmic ray tests, which must be carried out within a few weeks of the meteorite landing, before the radioactive elements have decayed. In two fairly recent cases, for example, such work has been hampered by ownership disputes. The St Marguerite meteorite which fell in north-east France in 1962 was transferred to the Paris museum for study, but when the analysis was half completed, the farmer on whose land it fell asked for the sample back, and the museum had no option but to return it. Similarly, when the Bovedy meteorite fell in Northern Ireland in 1969, half of it was lent to the Ulster Museum for analysis, but the other half was retained by the farmer. The museum has used up most of its sample in tests, and it is believed that the rest of the meteorite has found its way

to Armagh and is unavailable to the museum.

If Lord Cranbrook's bill becomes law, the British government will be following a precedent established in India in the 1870s. The Indian government then passed an act which gives the state the right to all meteorites which fall in India. The system of rewards to the finder that was instituted at the same time means that nearly all the meteorites that land in the country find their way quickly to the museums for study.

ARCHAEOLOGY

Sutton Hoo Revived

THE first presentation of the extensive new work that has been devoted to finds from the Sutton Hoo 7th century ship burial was given at a recent meeting of the Society of Antiquaries by Dr Rupert Bruce-Mitford, Keeper of Medieval and British Antiquities at the British Museum. It acted as a tantalizing curtain raiser for the publication of the definitive four volumes on the ship and its treasures, described by the distinguished Anglo-Saxon scholar Sir Thomas Kendrick as "the most marvellous find in the archaeological annals of England".

Dr Bruce-Mitford concentrated on objects which have been the subject of new work or about which modern studies have produced revised opinions. These include the design of the elaborately decorated helmet, the so-called "standard", the royal whetstone, numerous textiles (some of them of great richness and previously little discussed), the drinking horns, and some impressive wrought iron work associated with the large bronze cauldron. The cauldron has now been reconstructed from several thousand crumbled fragments. Two years of patient detective work at the museum, and some pre-war photographs, enabled a small firm of specialist ironmakers to work out how to make a reconstruction of a wrought iron chain which includes sections of plaited metal bars and even a reef knot.

During further study and laboratory work on the controversial large ceremonial object known as the standard, the correct attribution of the elegant little bronze stag mounted on a decorative ring has emerged. It is now definitely not associated with the standard but seems to have surmounted the royal whetstone. This strange carved red stone object, two feet in length, had previously been considered as a ceremonial object which was carried or laid horizontally. Fragments that match the broken "foot" of the stag's ring were detected at the end of the whetstone where something is clearly missing. It seems the royal whetstone was a symbol of royal authority and dignity, similar to a sceptre. It also now seems that the king commemorated by the Sutton Hoo ship burial was High King Redwald of Anglia—a lapsed Christian.

ENVIRONMENT

Preparing for Stockholm

ANYBODY in Britain will have the opportunity of influencing the British contribution to the United Nations conference on the environment in 1972, or so it seems from the plans outlined by Mr Peter Walker, Secretary of State for the Environment, this week. He has appointed four chairmen to assemble working parties which, it is intended, will gather information and advice from diverse sources and then mould them into a brief for Mr Walker to take to the conference as head of the British delegation.

He seems set to spend an exhausting two weeks in Stockholm, for much talk will surely be needed to fulfil the official purpose of the conference, which is to evaluate the present state of the world environment and discuss the principles on which future international action can continue and might be extended. The world environment has been made more manageable as a topic for discussion by dividing the agenda into three themes: the control of pollution, the human habitat and the management of natural resources. Three of Mr Walker's working parties are to be concerned with these topics, and the fourth will investigate the contribution which young people can make to the preservation and improvement of the environment.

The working party on the control of pollution is to be chaired by Sir Eric Ashby, who should have special expertise as the chairman of the Royal Commission on Environmental Pollution. Lady Dartmouth, who is to chair the working party on the human habitat, has had much experience in local government to help her in the contemplation of housing problems and the preservation of historic buildings. Mr Ralph Verney, who is to look after the management of natural resources, is a member of the Forestry Commission with experience of local government, finance and planning. The fourth working party is to be chaired by Mr Dennis Stephenson, the twenty-five year old director of a market research firm, who has been building up a reputation as a community worker and social researcher. He and his colleagues will be collecting the environmental views of all the young people's organizations they can find.

Mr Walker has invited anybody to send memoranda, evidence, ideas and suggestions to the four working parties, saying that he wants the British contribution to the UN conference to develop out of a "dialogue with the country". He also hopes that the pre-Stockholm cogitations will produce something of value for home consumption as well as providing the background for his speeches to the conference.

INDUSTRIAL TRAINING

Change of Role

THE industrial training boards have been under attack from several sides ever since they were set up in 1964. Small firms complain that they do not get value for the money they have to pay out on levies, many complain about the complex system of grants and levies and, perhaps most important, the financial instability of many of the boards has been demonstrated by the recent floundering of the Construction Industry Training Board. But at least one, the Engineering Industry Training Board (EITB), has shown willing to change, and other training boards are likely to follow suit. The EITB has drawn up a set of proposals for simplifying the present system of levies and grants, and for changing its own status to that of an advisory body.

The chief reason for these proposals is that the EITB believes that the present system of blanket levies and grants will have achieved its main objectives by 1973, and that the training boards should become advisers rather than heavy-handed persuaders. There can be little doubt that the system set up by the Industrial Training Act in 1964, in which training boards essentially extract money from all the firms under their particular

ambit and then reimburse them for expenditure on industrial training, has improved the level of training over the past few years. But there is still a real need for advice to firms on how to meet and identify their individual training needs.

The EITB therefore proposes to exempt from levies all those companies on its register whose payrolls are less than £25,000, and to develop its advisory role at the expense of the general grant scheme. But the board still recognizes the need for maintaining an adequate supply of transferable skills, increasing the supply of skills that may temporarily be in short supply—for example, systems analysts—and for encouraging firms to institute their own training schemes, particularly where this involves high initial outlay. How the EITB intends to achieve these objectives is less than clear from the proposals put forward so far. The board does suggest, however, that grants should be placed into two different categories: specific grants for those skills that are of concern to the engineering industry as a whole, and general grants for all other training. Small firms which pay no levies would be eligible for specific grants, but not for general grants. These proposals have now been circulated to the engineering industry, and a more detailed scheme is likely to be formulated by spring 1972.

Parliament in Britain

Euratom

IF Great Britain becomes a member of the European Economic Community, the British government should pay compensation for the existing scientific information made available by entry into Euratom. This request was made to the British delegation to the negotiations in Brussels last January, but Mr Nicholas Ridley, Under Secretary of State, Department of Trade and Industry, said that the request was turned down. The British delegation replied that although Britain intends to play a full part in Euratom, it believes that the request for compensation is not justified. (Written answers, February 1.)

Satellites

MR DAVID PRICE, Parliamentary Secretary, Ministry of Aviation Supply, said that on January 28, 1971, 2,139 objects were in orbit around the Earth. Of these, 419 were satellites. On the same date, there were seven probes in orbit around the Moon. Of the 429 satellites, 304 belonged to the United States and 105 to the Soviet Union, and of the seven lunar probes, three were launched by the United States and four by the Soviet Union. (Written answers, February 1.)

Influenza Vaccination

ROUTINE use of influenza vaccine is unlikely to prevent the spread of influenza, according to the Joint Committee on Vaccination and Immunization. Mr Michael Alison, Under Secretary, Department of Health and Social Security, said that the committee advised the Secretary of State for Social Services that the vaccines currently available give short-lived protection to about 40–60 per cent of the individuals vaccinated. Administration of vaccine to the whole community would therefore be an expensive operation of dubious value. Moreover, doctors have been advised that the committee believes that vaccination may be indicated for the protection of persons suffering from certain chronic diseases in whom influenza might aggravate their disability. (Written answers, February 2.)

Fowl Pest

MR JAMES PRIOR, Minister of Agriculture, said that the provision of compensation or other government aid to farmers who have lost their stocks because of fowl pest would reduce the incentive to maintain a high level of vaccination among flocks. He therefore rejected a suggestion put forward by Mr Edward Bishop that the government should revise its policy of not providing bridging aid. (Written answers, February 5.)

Miscellaneous Intelligence

IT now seems probable that there will be a revival of scientific relations between the Royal Society of London and the Chinese Academy of Peking. Some eager Fellows of the Royal Society are already wondering how to pack their bags. Old China hands are warning them that it is best to prepare for unexpected circumstances, boredom chief among them. One distinguished visitor to China has sad tales to tell of waiting in the same hotel for days on end for the interpreter who failed to show up.

STANFORD UNIVERSITY has an enviable reputation among American universities for its bravery in setting up so-called campuses abroad, and an enviable reputation among connoisseurs of English country houses in having acquired the lease of Cliveden, the old Astor home, from the National Trust as the British outpost of Stanford. The students seem to be careful lodgers. The swimming pool is out of bounds.

ALTHOUGH the troubles at Rolls-Royce may have emerged only recently, there have been signs of impending trouble for a long time. In retrospect, many journalists will be struck with the suddenness with which the company ostentatiously said nothing—and would say nothing—about

the use of carbon fibres in aero engines almost exactly three years ago. Visitors to the World Fair in Osaka (Expo 70) will no doubt have been surprised to see how faithfully this modesty was reflected in that exhibition.

TAMARAWS (*Anoa mindorensis*) can breathe again. The latest report from the International Union for Conservation of Nature and Natural Resources (IUCN Publication No. 18) says that the species is probably out of danger, thanks to the collaboration of several agencies in the Philippines including the Department of National Defence, the Special Forces team of the Philippine Constabulary, which lent some people and a jeep, and the Superintendent of the Sablayan Penal Colony, who gave the conservationists somewhere to stay. Spin-off so far includes the formation of the Philippine Wildlife Conservation Foundation, a radio programme called "It's a Beautiful World" put out on Wednesdays, and the sighting of twenty-four tamaraws in various groups. Without doubt the most remarkable achievement is a veritable change of habit—"tamaraws are no longer so shy . . . the animals seem to realize that they can now safely stay in the open, thanks to the campaign . . .".

NEW WORLD

Physicists on the Bread Line—Who Next?

by our Washington Correspondent

THOSE who dismiss as exaggeration the description of the scientific university system as squealing with pain under the present financial pressure had only to attend the meeting of the American Physical Society in New York last week to hear sounds of genuine anguish. The spectre of unemployment that has so far been an apparition for all but a few per cent of academic scientists is now beginning to materialize. Physicists registering with the placement service operated by the APS outnumbered prospective employers by more than 10 to 1. Surveys reported to the meeting cited the increasing difficulty of recently graduated BS and PhD students in finding a job, and an even bleaker situation for displaced older physicists. The bad news was amplified by the recriminations of a small group of younger physicists who blamed their elders for having failed either to foresee or to forestall the present crisis.

According to data gathered by the American Institute of Physics (of which the American Physical Society is the largest member society), 12 per cent of those who registered with the APS placement service in spring 1970 were still out of a job by the end of last year. A repoll of respondents to employment surveys conducted last year indicated that of those who had received no job offer by July 1970, 17 per cent of PhDs and 23 per cent of BSs were still jobless by the end of the year. Recently graduated PhDs who were known to be on temporary postdoctoral fellowships in December 1969 reported unemployment of 3 per cent. The AIP also polled physics department chairmen who report an average 18 per cent drop in graduate school enrolments this year, with high quality schools having cut back their graduate student intake far more severely than have others.

Another disturbing aspect of the employment situation has been documented for the APS by Lee Grodzins, professor of physics at MIT. Out of 202 "senior" physicists (those with PhDs gained before 1966) who registered at the spring 1970 placement service, only 60 per cent are now holding jobs that make extensive use of their physics. Of the rest, 7 per cent report they use their physics training a little, 10 per cent for background only and 5 per cent not at all. The remaining 17 per cent of the registrants are out of work.

From a name by name comparison of physics departments over the two most recent years, Grodzins estimates that

800 new PhDs found their way into the system but academic faculties grew by only 100 members over this period, suggesting that older physicists are being squeezed out. "These physicists who involuntarily leave their profession are not part of the losers of society but are among the most talented of their generation," Grodzins told the New York meeting of the APS last week. "If science funding does not return to a reasonable growth pattern soon, we will have to contend with a wreckage in careers which took a decade and hundreds of thousands of dollars to train."

Grodzins also predicted a "chronic surplus" of PhDs throughout the 1970s, which is already apparent among those who graduated last year. From a census of the 1,500 physicists who received their PhD in 1970, Grodzins estimates (the census is not yet complete) that 15 to 20 per cent took up faculty positions and another 40 per cent received temporary postdoctoral appointments. About 10 per cent entered industry and another 5 to 7 per cent joined government laboratories. The remaining 30 per cent or so were largely wasted; 10 to 15 per cent left the country and a similar proportion left physics. The Army accounted for 2 per cent and up to 5 per cent are unemployed.

The implication of these sombre statistics was reinforced by Brian B. Schwartz, also of the MIT physics department, who puts the maximum demand for physics PhDs over the next decade at about 800 a year (the present output is 1,500 a year), an estimate which implies on average each of the nation's 170 physics graduate schools should turn out no more than five PhDs a year, and each professor should produce one PhD thesis student every six years. Schwartz's estimate of annual demand assumes that 400 PhDs will be needed in physics faculties each year for the next decade (a figure based on the expected size of the student enrolment), 250 in industry (assuming a 4 per cent growth in employment of PhD physicists) and 150 in government laboratories (also assuming a 4 per cent growth rate). Schwartz denies the hopes that there will be a large demand for PhD physicists in environmental subjects: "At MIT we are grinding out experts on urban problems, transportation, health care—you name it, we will have a PhD programme in no time," he told the APS meeting to discuss graduate education in physics. "Physicists will not get these kinds of jobs because they will be

competing with people specially trained in them." Schwartz considers it ridiculous that most of the 170 graduate schools should try to produce PhDs in each of the five main disciplines of physics: "It would be far wiser to reduce the total number of graduate schools and reduce the number of specialties in each institution. . . . Unfortunately, it seems most unlikely that graduate schools and faculty members will cooperate to produce fewer PhD physicists."

The concern expressed by Grodzins and Schwartz was underlined by the relative attendance of employers and prospective employees at the placement service conducted by the APS during last week's meeting. The number of employers attending the placement service dropped from 250 in 1966 to 100 last year; this year there were only 52 employers of all kinds with jobs to offer to the 700 physicists seeking employment. The American Physical Society has been sufficiently concerned about the employment situation to hire a full time job consultant, Raymond W. Sears, who started work this year.

But the attitude of the society seems to be to play down the seriousness of the present and impending job shortage; APS officials argue that the current situation may be only a "short term fluctuation" and that, if enrolments are restricted now, there could again be a shortage of physics manpower should the economy pick up and the demand for physicists resume its expansion. "The job situation is serious and worse than last year," was all Mr Sears would admit to. "No, I would not call it a catastrophe, but I wouldn't say it was not a catastrophe either."

Whether a catastrophe or not a catastrophe, the young physicists closest to the job-hunting market made clear their apprehensions about the situation. Through a surprising lack of foresight, the organizers of the APS programme had made no provision for discussing the employment situation which exercised the interest of so many of their members, and people's worries were forced to the surface mostly during a session chaired by Dr Frederick Seitz, president of Rockefeller University, which was intended to discuss the education rather than the employment prospects of physics graduates. To the young physicists in the audience, many of whom seemed not to be aware of this distinction, the speeches from the platform appeared callous and irrelevant, while to those on the platform the remarks of their audience

must have seemed discourteous and beside the point.

Speakers from the audience, several of them sporting the badge of the radical Sespa movement, did not go out of their way to lessen the generation gap. The members of the panel had to listen to denunciations of their personal backgrounds and connexions with the defence industry or government—Seitz was at one point called the “dowager-empress of the military-industrial-academic complex”—and did so with stoic forbearance.

More apposite were the complaints of those who rebuked the leaders of the physics community for not having predicted the impending job crisis. “The leaders of the American Institute of Physics and the American Physical Society have showed poor judgment in this area and I don’t think they should be our leaders any more,” was one comment from the floor. Another speaker objected that “the people who got us into this mess are being turned to again to get us out of it”.

The dissenters at least had a constructive, if not wholly altruistic, proposal to offer, known as the Arden House proposal. Devised by a meeting of graduate students at Arden House of Columbia University last November, the scheme would insert into the PhD producing system the kind of direct feedback mechanism which it is at present lacking. The essence of the proposal is that graduate departments should divert the money that would otherwise have gone to train new graduates into supporting those of their graduated PhDs who have not yet found jobs.

If there is any justice in the young physicists’ jibes at their elders’ lack of foresight, it is because the present crisis and part of the reason for it was predicted as long ago as 1965 but ignored by the physics community along with most other people. Allan M. Cartter, an economist then working with the American Council on Education and now chancellor of New York University, predicted that a surplus of PhDs (in all subjects) would begin to emerge in 1969, becoming significant during 1970–74. Cartter’s projections, in essence, were based on the simple demographic argument that the lower fertility rates of the early 1960s would result in a smaller college enrolment in the 1980s, and hence a smaller faculty; before that time, however, beginning in 1967, the rate of increase of the student body would drop, leading to a smaller absolute demand for teachers. Cartter relates how, in 1965, these predictions were not ignored but subjected to outspoken public criticism by people in the Office of Education and the National Science Foundation, who felt that they would jeopardize federal funding for graduate education. “My argument then, as now”, Cartter said in

Table 1 Estimates by Allan M. Cartter of New Faculty Required to Maintain Quality, and New Doctorates Available; Actual and Projected, 1965–85

		A New faculty needed (3 year average)	B New faculty with PhD Needed (70%)	C New PhDs	D Ratio C/B
Chemistry	1965	722	505	1439	2.9
	1970	703	492	2030	4.1
	1975	825	578	2290	4.0
	1980	678	475	2888	6.1
	1985	–53	–37	n.a.	—
Physics	1965	583	353	994	2.8
	1970	568	341	1617	4.7
	1975	666	400	1997	5.0
	1980	550	330	2680	8.1
	1985	–43	–26	n.a.	—
Biology	1965	1535	1136	1928	1.7
	1970	1495	1106	3480	3.1
	1975	1753	1297	4395	3.3
	1980	1446	1070	5774	5.4
	1985	–113	–84	n.a.	—

a paper given to the AAAS meeting in Chicago last December, “is that it would be much better to be objective and attempt to make the case for the support of graduate education on its merits, rather than to ride the scarcity myth another two or three years and then have Congress react strongly once the apparent crisis was past”.

Cartter’s most recent estimates suggest that over the next decade only one doctorate in four will find suitable academic employment and in the 1980s this proportion could fall to less than one in ten. As for specific subjects in physics, the number of new PhDs produced will exceed the number of new PhD faculty required by a factor of 5 in 1975, rising to an eight-fold excess by 1980. For chemistry the ratios for 1975 and 1980 are 4 and 6 respectively, and for biology 3.3 and 5.4. Throughout the 1960s the ratio of PhD supply to demand in these subjects has typically been about 2, leaving roughly half of the output to find non-academic employment (see Table 1).

The executive committee of the American Physical Society announced at last week’s meeting that being conscious of the difficulties of its unemployed members, and aware of how much they prized being members of the APS, it had taken concrete action on their behalf. The action is that out-of-work physicists, provided that they have been APS members for at least one year, may defer (not forgo, defer) the payment of their membership dues for 1971 until they get a job. The APS executive committee might have earned more of a reputation for concrete activity had it listened to Allan Cartter in 1965, even though he wasn’t a physi-

cist. Nevertheless, the society, through the AIP, seems to have done as much as it could over the past year to make good its neglect of manpower studies.

For the long term future Cartter believes that the present course will result in overproduction of science PhDs of about one-third during the late 1970s, and of perhaps one-half in the 1980s, as far as concerns the types of employment considered suitable for PhDs in the past. An increasing proportion of the surplus will be employed in jobs remote from their training, the price of the surplus being not unemployment but underemployment. “Associated with it frequently are personal disappointments, and a gradual erosion of hard-earned skills and intellectual agility”, Cartter says. “There is a social cost—probably about \$50,000 directly invested in the individual with the PhD. . . . But the human cost in unfulfilled expectations and discouragement may be even more important”.

Cartter commends the scientific professional associations such as the American Institute of Physics for the studies of manpower problems which many have recently set up, but regrets the absence of a group to coordinate these separate efforts. As for government policy, the recent reductions in federal support of graduate education seem designed to starve the system back to more appropriate levels, which Cartter sees as “a kind of Malthusian market adjustment” favouring the survival of the least fit since “it is the very universities that have made the greatest commitment to graduate . . . education, and who have served national research needs most extensively, that are reaping the whirlwind”.

NATIONAL BIOLOGICAL CONGRESS

Confusion, Hope and Gloom

from a Special Correspondent

Detroit, November 1970

THE first National Biological Congress was, like the curate's egg, good in parts. Sponsored by the American Institute of Biological Sciences and by the Federation of American Societies for Experimental Biology, the meeting was held in Detroit on November 7-10. It was announced as a major effort in scientific communication between biological investigators presenting their findings in basic science symposia, on the one hand, and legislators, administrators in industry and government and the general public on the other hand. Students and college teachers were to bridge the gap, and discussions of public issues and of basic science were to be the main concern of the congress, with its overall title "Man and Environment". Although some of the science and some of the discussion was first rate, the meeting was not the success which we had all hoped it would be. Only occasionally could one see signs of hope for the future and the role of biology in that future.

During the four days, eleven sessions were held, and forty organizations and scientific societies participated. There were eight simultaneous sessions, fortunately in rooms close to each other, but clearly this report can be no more than one individual's impressions. Some at least of the scientific presentations were outstanding and exciting, but the 2,800 who registered—a fair proportion of them high school and college students—only rarely showed enthusiastic involvement.

The keynote speech was by Dr William D. McElroy, the director of the National Science Foundation, on "The Role of Science in Public Affairs". This reporter has the impression that Dr McElroy's speech cast considerable gloom over the biologists present at the congress, because it seemed to urge them to follow the apparent official trend towards goal-oriented research, to do socially useful research at the expense of fundamental work. The scientists who were present were unhappy to hear such words from the supposed champion of fundamental research.

The three mornings of basic science symposia presented a smörgasbord where one might "simultaneously partake of sessions on genetics and molecular biology, metabolic transformations, cellular and subcellular structure and function, developmental biology, function of tissues and organs, behavioural biology, ecology and also evolution and population biology". In genetics and molecular biology, sessions were held on DNA replication and transcription, protein synthesis and on the three-dimensional structure of macromolecules. All of

these were well attended and presented a well organized if somewhat too specialized picture of the current thinking.

In developmental biology the subjects ranged from genetic aspects of development and gene regulation in developing embryos to the growth of whole plants from single cells. Particular attention was paid to developmental abnormalities—a subject very much in line with the overall intention of the congress—with a description of the genetic approach to causation of developmental defects in mice and men by Dr Salome Waelsch and a discussion by Dr Barry Pierce of the cancer problem in terms of the failure of stem cells to differentiate.

In behavioural biology, the papers included discussion of alcoholism and drug addiction as well as behavioural genetics with, for example, an excellent lecture by Dr Julius Adler on the genetics of chemotaxis in bacteria. What may well be a break-through paper in behavioural biology and neurobiology was Dr Seymour Benzer's description of how he obtained behavioural mutants of the fruit fly *Drosophila*, how he constructed mosaics with some of these mutants and how he analyses the mutant phenotypes. It may well be that the use of mutants as practised by Benzer in *Drosophila* and by other investigators will prove to be the most incisive method for cracking the complexities of neurophysiology and of behaviour. At the other end of the scale, a session was devoted to learning and development in the human neonate. For example, a paper on the role of the mother in influencing the early development of infant behaviour was read by Dr Evelyn Thoman, presenting a serious attempt to investigate the effect which modes of handling of the infant has on such parameters as cessation of crying and degree of visual alertness.

One specific occasion when a basic science symposia was really crowded and full of excited young people was, not unexpectedly, a session in the ecology series on population and resources chaired by Dr Paul Ehrlich. Population control was the subject of speakers such as Drs Judith Davis, Paul Ehrlich and Christopher Tietze. Most of the audience were students and their enthusiasm for the personalities and for the subject was enormous. Within the limits of observation of this reporter, this session and an afternoon session on "Molecular Biology and the Future of Man" were high points of enthusiasm. The latter session was organized by the American Society of Biological Chemists and the Biophysical Society. A calculation of benefit-cost factors led Dr John Platt to the suggestion that the

most helpful thing that biology can do for mankind would be the development of better contraceptives and contraceptive techniques and the scientific selection of champion farm animals and crops. There was also the feeling expressed by Dr James Bonner that molecular biology itself cannot solve biological problems in our environment, and that only society acting as a whole can do this.

The evening sessions were devoted to three all-congress symposia on ecology, disease and chemicals. They were held in the huge Cobo Hall Arena, with the public of Detroit invited. Although the topics were certainly important enough to affect everybody in the community, the people of Detroit stayed away, perhaps watching the proceedings at home on television. As a result, only a small portion of the huge arena was filled, largely with those members of the congress able to face the third meeting of the day. • The proceedings of the ecology and the disease symposia, although rather formal, were in part very interesting. One remembers Dr LaMont Cole's statement on the subject of environmental pollution—that it was irresponsible to set up nuclear power stations with present technology, not only because of the problems of heat pollution and the disposal of radioactive wastes, but because present day plants are so inefficient that they waste more than 90 per cent of the energy of ^{235}U , a limited natural resource. One remembers also the statement by Senator Packwood from Oregon that in the areas of ecology, pollution and the environment the public must make up its mind as to what it wants and then impose its will on Congress. Speaking on the subject of genetic disorders, Dr Alec Bearn struck an optimistic note and discarded the old adage that "the only cure for a genetic disease is the grave". He looked forward to a time when prenatal investigation of the foetus in families with a high genetic risk would lead to therapeutic intervention, either treating or aborting a defective homozygote. The audience participated by sending in questions to be put on the platform and by occasional applause, while the TV cameras looked on in silence.

To repeat, this report is only one person's view of an ambitious and multifaceted event. While many individual talks were informative, enjoyable and sometimes exciting, the overall impression at the congress was one of confusion, occasional hope and, certainly, occasional gloom. This was one of those occasions where the whole is less than the sum of its parts. The avowed intentions of the organizers of this congress are unquestionably worthwhile; whether the congress succeeded and whether it should be repeated is doubtful.

(This article has been accidentally delayed.)

NEWS AND VIEWS

How to Find Needles in Haystacks

LIKE the objects to be found at the end of the rainbow, the superheavy elements have long been regarded enviously but with disbelief. The report from the Rutherford High Energy Laboratory on page 464 of this issue of *Nature* may change the balance between hope and scepticism. What Dr C. J. Batty and his colleagues at Chilton, with their associates at Manchester and Risley, have done is to bombard a tungsten target with a 24 GeV proton beam in the CERN accelerator in circumstances in which they have reason to expect or at least to hope that the nuclei of elements with atomic number well above the present limit of 105 may have been produced. By careful radiochemical treatment of the irradiated targets, they have separated those fractions which seem chemically related to mercury. Among the residues, they have found traces of alpha activity which cannot be accounted for by the known activities of isotopes of lighter elements, and their material seems also to contain fission activity which cannot easily be explained away. The question now is whether the inference can be substantiated that the nuclear activity not so far identified can be attributed to the artificial element 112. The authors of the research are properly undogmatic in their interpretation of their results, for this is clearly a field in which the reality or otherwise of each new element beyond the known limit must be confirmed by a string of independent observations. No doubt the team which has now provided a hint of the existence of element 112 will itself have much to say.

The device of bombarding tungsten with protons in the hope of making superheavy elements is not as much a matter of hit or miss as it might seem to be. By now, it is clear enough that the manufacture of ever heavier nuclei by the successive addition of nucleons is bound to be subject to the law of diminishing returns. This indeed is why Professor G. L. Ghiorso's group at Berkeley has invested ingenuity and money in a linear accelerator for producing high energy beams of atomic nuclei such as helium, carbon and oxygen, but even here the problems of producing sufficiently energetic beams of atomic nuclei sufficiently heavy to span the gap between, say, uranium and element 112 are formidable to contemplate. This is why the new work at CERN has been designed around the expectation that in the interaction between fast protons and tungsten nuclei, the recoil nuclei will often carry between 1.0 GeV and 5.6 GeV of energy. But the potential barrier between tungsten nuclei is reckoned to be about 1.0 GeV, so that there will often be occasions when pairs of tungsten nuclei are made to coalesce. By the loss of particles from such an assembly, so the argument goes, a whole range of lesser atomic nuclei should be produced. What Dr Batty and his colleagues have done is to search for radiochemical evidence of the existence of element 112 which should lie beneath mercury in the periodic table and which may be lent stability against decay by radioactivity and fission by the symmetry of its assemblage of nucleons.

Quizzical experimenters will be most of all concerned with the radiochemistry, at once intricate and delicate. The amounts of material involved are in the best tradition of radiochemistry, with microgram quantities of mercury

being added as a carrier. The target carried most quickly to the laboratory bench was irradiated with 7×10^{17} protons, one for every half a million tungsten atoms. After the separation of mercury and other chemically related elements, the presence of something unexpected was indicated chiefly by the existence of a peak in the spectrum of alpha radiation which cannot be attributed to the decay of a known isotope of some other element likely to have been present. The authors themselves point out that the possibility of contamination with thorium is a potential pitfall, one they consider they have avoided. No doubt other people will describe other potential snags. That, inevitably, is how the interpretation of experiments like this must be carried out. To be sure, the signs that there may be spontaneously fissile nuclei in the products of the experiment will help to bolster up the view that something new has been uncovered. In the long run, however, the belief that element 112 has indeed been manufactured in the CERN accelerator will be best of all sustained by a thorough characterization of the isotopes concerned. The estimate of a half-life of 500 years for the new material which Dr Batty and his colleagues put forward is at this stage more a point for further discussion than a potential test of what the theoreticians have calculated.

The notion that there may be nuclei containing many more particles than uranium nuclei which are nevertheless comparatively stable against spontaneous fission and radioactive decay owes its existence to the attempts in the past fifteen years to account for the energetic properties of the nucleons in familiar nuclei by means of comparatively stable shells of particles comparable with, although more complicated than, the electron shells surrounding atomic nuclei. In familiar circumstances and for stable nuclei, for example, the predictions of the shell model work well enough. Just as the electronic shells of atoms with two and ten electrons are exceptionally stable, so too are nuclei such as those of the isotopes oxygen-16 and lead-208, for eight protons and eight neutrons, or 82 or even 126 of either, constitute magic numbers in the construction of atomic nuclei. One result is that, for example, nuclei with atomic number 50 (another magic number) are expected to be exceptionally stable, which goes a long way to explain why tin, the element concerned, is so prolific in its isotopes. Unfortunately, but predictably, the calculation of the magic numbers is much more hazardous with nuclei than with electronic shells, for an assembly of nuclear particles in a nucleus must provide its own self-attractive potential energy, whereas comparatively simple atomic systems have a central dominating nucleus to provide a pattern of potential energy.

One complication is that for heavier nuclei, the magic numbers probably differ for neutrons and protons and are in any case only known approximately. In circumstances like these, the difficulties in calculating and predicting nuclear properties must necessarily be formidable. On some occasions the uncertainties have been so many that only the faithful have kept the dream of the superheavy nuclei alive. The new results should give them courage.

PLIO-PLEISTOCENE

Magnetic Definition

from our Geomagnetism Correspondent

ALTHOUGH much work has been done on the geomagnetic stratigraphy of deep sea sediment cores, very few similar studies have been made of marine sediments now exposed on land. The reason for this is partly because land-based sediments have often been subjected to uplift and erosion (which complicates palaeomagnetic measurements), partly because exposed sections are not usually very long and partly because continental sediments seem to be far more prone to chemical alteration than their deep sea counterparts. Yet, as Kennett *et al.* have demonstrated (*Science*, **171**, 276; 1971), a good land section of sedimentary rock offers the opportunity not only for magnetic stratigraphy determination but also for correlation with geological stratigraphy and palaeoclimatic indicators and for throwing some light on that vexed question of where to place the Plio-Pleistocene boundary.

The geological controversy aroused by the Plio-Pleistocene boundary in the past century is one of those things which most geophysicists find incomprehensible. Indeed, in this age of the radiometric date, the so-called absolute date, it is difficult to see why what is, after all, an arbitrary nomenclature really matters. That it matters to geologists would seem to be a hangover from the days when the chief contribution of geology was to place a seemingly infinite number of unrelated facts into some sort of manageable framework. The irony is, of course, that although geophysicists care very little where the Plio-Pleistocene boundary is, or whether it should exist at all, it is geophysical measurement which is now making a much more precise—and, dare one say it, sensible—definition of this boundary possible.

And so to Kennett *et al.*, who have been proving this very point by measuring the magnetic stratigraphy of a section of Pliocene and Pleistocene sediments in New Zealand. Comparison of the magnetic reversal pattern over the past three million years with that determined from continental rocks has enabled an absolute chronology to be placed on this particular sedimentary sequence the oxygen isotope and foraminiferal palaeotemperature curves of which had already been determined by Devereux *et al.* (*Earth Planet. Sci. Lett.*, **8**, 163; 1970). Using the new chronology it is now possible to see that the first major Pliocene cooling occurred 2.50 to 2.30 million years ago (spanning the Gauss-Matuyama magnetic boundary), that the second coincides with the lower part of the Olduvai event (2.13 to 2.10 million years ago) and that a third cooling (1.98 to 1.88 million years)

correlates with the upper part of the Olduvai.

The implications of the new magnetic data for the position of the Plio-Pleistocene boundary are necessarily involved and not easily stated in a few words. But the situation is basically this. The Plio-Pleistocene boundary in New Zealand has been defined in two ways—climatically, based on the first severe cooling, and stratigraphically, based on correlation with the Calabrian stage in southern Italy. As far as the climatic definition is concerned, Kennett's new chronology clearly shows that the first major cooling took place about 2.5 million years ago and thus on any reasonable scheme falls in the Middle Pliocene. To be sure, this begs the question of how the Pliocene is defined in the first place; but taking geological and palaeontological constraints into

account, all geologists would admit 2.5 million years as Pliocene. The climatic definition of the Plio-Pleistocene boundary in New Zealand is therefore wrong.

The correlation with the Calabrian type section in Italy is based on the first appearance of *Globorotalia truncatulinoides* which in New Zealand is now seen to coincide with the base of the Gilsa magnetic event at 1.79 million years. By comparison, the Plio-Pleistocene boundary within the Calabrian type section, based on the extrapolation of sedimentation rates, is 1.8 million years old. Thus, by what presumably amounts to a coincidence, the accepted type Plio-Pleistocene boundary can now also be defined in terms of magnetic stratigraphy. Clearly this will make it much easier to trace this boundary around the world.

Microseisms and Storms at Sea

MICROSEISMS are small tremors in the Earth's surface with amplitudes of up to 10 μm ; those of frequency 0.1 to 0.5 Hz are very closely associated with sea waves near the coast or in a storm area up to 2,000 miles away. In next Monday's *Nature Physical Science*, T. D. Krishna Kartha describes his work on the variation of velocity (refraction) of microseisms approaching Cochin in southern India. Refraction diagrams have already been produced for the western approaches to the British Isles, the seas around Bermuda and the seas to the south of South Africa.

The frequency of microseisms is usually about twice that of the associated sea wave and increases near the coast as the swell frequency increases. For a long time it was not clear how sea waves out at sea could generate microseisms because the pressure of the sea wave vanishes at depths greater than half a wavelength (about 150–300 m). Longuet-Higgins showed, however, that in the case of a stationary wave generated by two identical progressive waves travelling in opposite directions there is a second order pressure effect which does not vanish with depth and which fluctuates at twice the original wave frequency (*Phil. Trans. Roy. Soc., A*, **243**, 1; 1950). This result was generalized to the interaction of waves of the same frequency but different amplitude, and it can be shown that there is still some second order effect when any two waves of equal frequency but different amplitudes and directions act together.

These conditions are clearly satisfied when swell is reflected off the coast, but they also exist in the case of a fast moving storm which can create sequences of

waves travelling in different directions.

Microseisms with the same frequency as the sea waves have also been detected recently; these are generated near the coast—where the wave pressure is still appreciable—but can be propagated over large distances. (It can be shown that even though the wavelength of the sea wave is usually less than 700 m, the pressure effect spreads over a wide range of wavelengths including that of seismic waves (about 20 km).)

Microseisms are of practical importance in predicting the onset of a storm in areas where meteorological observations are scarce as, for example, in the Pacific Ocean and the Indian Ocean; Kartha has dealt specifically with the latter. The microseism frequency gives the wave frequency and hence the wind speed in the storm; microseisms are also directional to some extent and, by using records of three components of ground movement, the azimuth of the storm can be found together with the actual position if two or three measuring stations are used. If the position of the storm is to be located, it is important to allow for refraction of the microseisms. Assuming that they are waves of the Stoneley type formed between the surface of the Earth and the sea, their velocities can be calculated; these seem to depend to a great extent on the sea depth, particularly if this is between 100 and 1,000 fathoms. Refraction diagrams can be drawn from the velocity measurement and these are useful not only to make corrections for direction of approach, but also to show up focusing or divergence effects and possibly to indicate that a storm acting from one direction is more powerful than one from another direction.

LASERS

Isotope Separation

from our Molecular Physics Correspondent

THE announcement of a successful separation of hydrogen isotopes by selective laser-induced chemical reaction (S. W. Mayer, M. A. Kwock, R. W. F. Gross and D. J. Spencer, *Appl. Phys. Lett.*, **17**, 516; 1970) will carry a stage further the reputation of the laser as a magic wand which can be pointed with startling results at practically all things physico-chemical.

This new application, which follows close on the development of a continuous-wave hydrogen-fluorine laser in the same Aerospace Corporation group (see *Appl. Phys. Lett.*, **16**, 386; 1970), underlines more than any other so far the now complete involvement of physics with chemistry in the field of continuous-wave gas lasers. The isotope separation is achieved by irradiation of mixtures of ordinary and deuteromethanol and bromine with 0.44 eV (2.64 μm) photons at a total power of about 100 watts (6×10^{20} photons per second). Of this some 50 per cent is absorbed in the band characteristic of —OH groups whereas almost none is absorbed by the deuterio-alcohol. Because the activation energy for attack of CH_3OH by Br_2 is only some 0.27 eV (25 kJ mol $^{-1}$) the reaction of this can be made virtually specific at laboratory temperature. Mayer and his colleagues do not actually separate the deuterated fraction but their absorption spectra show beyond doubt that the composition of the gas remaining indicates an enrichment from 50 per cent to more than 95 per cent CD_3CD .

It is important to notice that the selectivity exploited in this experiment depends only on the careful matching of the laser frequency to the vibration of the —OH bond and involves no kind of kinetic isotope effect. At the same time, because the energy of the photon is deliberately kept low, there is no actual photolysis of the alcohol and no dissociation of the bromine leading to the photo-bromination reactions familiar at shorter wavelengths. In fact, the process occurring is not easily labelled by any of the terms currently in use in photo-chemical kinetics—a sure indication of its originality and potential application. The authors, whose outlook, one imagines, is not primarily chemical, choose the rather misleading term “photocatalysis”—a better description might be simply “specific photo-activation”, in line with thermal, chemical, electron impact activation and the like.

Unfortunately it does not seem likely that such a clean separation will be possible with heavier isotopes, though, given chemical ingenuity and some form of recycling arrangement, the method could be competitive with others currently available.

Whether or not this is so and whatever name comes to be given to the experiment, it will almost certainly become a classic of chemical kinetics in its own right for, forgetting about isotopes altogether, the prospect of pumping infrared energy at such power into a specific vibrational mode of a reactive molecule opens the way to the kind of observation of which experimental kineticists dream. I feel sure that we shall begin to see the fruits of this from a number of different laboratories before long.

FIBRES

Reinforcing Boron

from our Materials Science Correspondent

A STUDY has been made recently (J. O. Carlsson and T. Lundström, *J. Less-Common Metals*, **22**, 317; 1971) of the efficacy of a group of solutes consisting of all 3d transition metals and copper, on the indentation hardness of β -boron. All the solute metals—Sc, Ti, V, Cr, Mn, Fe, Co, Ni and Cu—were arc-melted with boron at a concentration of 5 atm per cent, which in each instance was sufficient to produce a saturated solid solution (though the saturation concentration was not determined). The unit cell volumes of the solid solutions were determined and plotted against microhardness; the two variables were found to be closely

correlated. The maximum increase in hardness of about 20 per cent relative to pure β -boron was obtained for Cu, Sc and Mn solutes, corresponding to 1–2 per cent increase in unit cell volume.

In view of the importance of boron as a fibre-reinforcing metal, and because single crystals of boron are readily grown when transition metals are dissolved in it, Carlsson and Lundström suggest that alloyed boron should be investigated with a view to obtaining fibres or perhaps whiskers of enhanced strength.

It was recently reported from the National Physical Laboratory (S. Allen *et al.*, *Nature*, **224**, 684; 1969) that carbon fibre can be strengthened by alloying it with a little boron. The findings now reported for boron containing metal solutes suggest that boron fibres alloyed with carbon (assuming that any solubility exists) may turn out to be particularly strong.

NIAGARA

Regressing Falls

from our Geomagnetism Correspondent

THE Niagara River, like a good woman, is unusually constant. For one thing, no great uncontrolled river joins it above the principal (Horseshoe) falls; and in the drainage basin there is not much variation in discharge because upstream

Structural Inorganic Chemistry

ORGANOTRANSITION metal chemistry has been a definite growth point in chemistry during the past decade. The related themes of the reactivity of ligands coordinated to metals and the stabilization of unusual and reactive organic moieties through coordination are of particular importance because of their implications for mechanisms of homogeneous and heterogeneous catalysis. Diffraction methods in parallel with synthetic and mechanistic studies have provided precise data on the geometries of coordinated unsaturated ligands and the past three years have seen several attempts to evolve a generalized bonding theory. The molecules of real chemical interest are too large for any non-empirical quantum mechanical calculation of their energy levels and structure to be attempted, so much reliance has to be placed on symmetry arguments; fairly comprehensive calculations are at present restricted to such molecules as tetracarbonyl nickel. There are obvious shortcomings in this type of approach but the latest discussion by Mingos in next Monday's *Nature Physical Science* goes a long way to rationalizing the structural results.

The first order Jahn–Teller effect, which

comments on the removal of orbital degeneracy in molecules or ions of high symmetry, has been used widely by inorganic chemists to discuss stereochemical distortions in transition metal complexes. Pearson has recently taken up earlier work by Bader and Longuet-Higgins on the second order Jahn–Teller effect, which can make useful predictions of the consequences of “mixing in” electronically excited states on the ground state geometries of molecules. The importance of such admixture in organometallic molecules has been discussed by Mason, and now Mingos shows how simple symmetry considerations of the donor and acceptor orbitals of a Lewis acid-base pair give a clear indication of which nuclear rearrangements are favoured by ligands on coordination. The theory is based on symmetry alone, so it cannot discuss such matters as bond length variations in coordinated olefins and dienes as a function of the metal, the remaining ligands in a molecule and ligand substituents. But the general approach, linked with earlier theories, may prove as useful for organometallic molecules as Walsh's rules have been for simpler systems.

lie four large natural storage reservoirs—Lakes Erie, Huron, Michigan and Superior. So the flow of the Niagara River is almost constant, and must have been so for several thousand years at least. To be sure, sometimes westerly winds can drive the water of Lake Erie towards the entrance to the Niagara causing a 50 per cent increase in flow; but this is negligible as rivers go. The flow of the Ohio River, for example, varies by a factor of 313 within the year.

And the Great Lakes control not only the flow of water but also the sediment content. The only sediment carried by the Niagara over Horseshoe Falls is that picked up downstream of Lake Erie; and that is very little indeed. Moreover, the river flows through a plain except close to the Horseshoe Falls where, on the Canadian side, the ground rises to about 150 feet above the crest. As a result, the crest of the Falls has remained at about the same elevation throughout its retreat up the Upper Great Gorge. In short, the Niagara is an almost sediment-free river of almost uniform flow which falls over a crest of constant height.

All of which suggests that the rate of regression of the Horseshoe Falls as a result of erosion should have been more or less constant for about 4,200 years, or since the carving of the Upper Great Gorge began. Yet as Philbrick points out (*Bull. Geol. Soc. Amer.*, **81**, 3723; 1970), historical surveys made since 1842 show the rate of regression to have been very variable. Why should this be so? To explain it Philbrick looks to the curious bottom profile along the Upper Great Gorge. Recent surveys have shown the river bottom to be a series of basins, or plunge pools, which are progressively lower in elevation in the upstream direction. In a constant geological environment and with a uniform flow of water, the only conceivable reason for a series of well defined plunge pools would be a series of near-zero regression rates. In other words, the Niagara Falls must have remained more or less stationary for certain periods (during which the plunge pools formed) between which the regression rate was comparatively high. And this must have been going on not only since 1842 but for thousands of years.

From an examination of historical descriptions and sketches of the Horseshoe Falls, and by comparing Niagara with other falls in New York State, Philbrick concludes that the rate of regression of the fall crest is critically dependent on the crest's shape. When it is in the form of a horizontal arch (U-shaped), as the Horseshoe crest is today, the rate of erosion is very slow. But when a V-shaped notch develops in the arch the erosion speeds up. In this sense the horizontal arch is the much more stable configuration; and the accompanying lack of regression gives the

plunge pools time to form. The horizontal arch is not perfectly stable, however, because of jointing in the crestal rocks. If one of the slabs formed by the joints happens to break away, a notch quickly develops and the rate of normal erosion increases until the crest returns to the arched form.

All this suggests, of course, that the reasons for the variable regression rate are neither strictly geological nor hydrological. So Hubbard (quoted by Philbrick) has carried out a photoelastic study of laboratory models of the Horseshoe crest configuration in 1819 (a deeply notched arch as shown in an early painting by Van der Lyn) and 1966 (a broad arch). This clearly shows that the stresses at the apex of the 1819 notch are about three times as great as those at the equivalent point of the 1966 arch. Because higher stresses will lead to faster erosion under the constant flow of water, the laboratory evidence clearly confirms Philbrick's field deductions.

Philbrick's work is not only of academic interest but is part of a very practical programme to find ways of halting the erosion of the various falls in the Niagara area. Which is a pity. It would be nice to think that the Horseshoe Falls will go on retreating from the brash commercialism which currently surrounds this otherwise beautiful natural phenomenon.

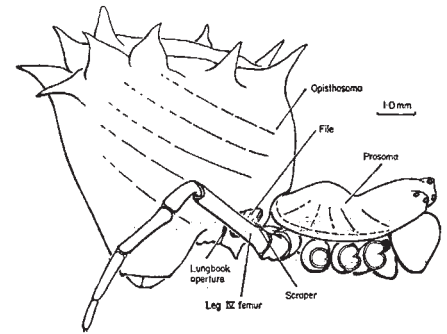
Electric Fields after Lightning

THE enhancement of the Earth's electric field associated with lightning flashes was first studied and discussed as long ago as 1920 and a number of different recovery mechanisms have been proposed. Illingworth and Wormell publish the latest contribution to this topic in next Monday's *Nature Physical Science* and they are able to make several fairly definitive conclusions about the recovery mechanisms. They measured the field recoveries after 672 lightning flashes seen at Cambridge, and found that their results fell naturally into two categories in which the field changes were greater or less than $1,000 \text{ V m}^{-1}$.

C. T. R. Wilson was the first to point out that the field recovery seemed to be approximately exponential after a flash; he suggested that there were two alternative explanations, one of which required a constant cloud charging current (after the flash) accompanied by a dissipation current proportional to the total thundercloud charge. His alternative idea was that the charging current might reduce as the charge on the cloud grew (*Phil. Trans. Roy. Soc., A*, **221**, 73; 1920). More recently, Holzer and Saxon (*J. Geophys. Res.*, **57**, 207; 1952) have found that the field of a charged thundercloud is profoundly modified by an atmosphere whose conductivity varies

ARACHNIDS

Files and Scrapers



Micrathena gracilis showing the position of the stridulatory file and scraper (from *J. Zool.*, **162**, 482; 1970).

INCESSANT chirpings of crickets are familiar sounds in the heat of a dry summer's day. The sound, called stridulation, is characteristic of members of the insect family Orthoptera (although it is also found in certain other insects), and is produced by the rubbing together of certain modified surfaces. In the cricket, for example, each modified forewing or tegmen bears a rasping organ or file and a hardened area or scraper, and these structures are worked backwards and forwards against each other thus vibrating the specialized resonance area of another part of the tegmen.

exponentially with height and that, at a distance of about 30 km from a storm, the size of the field due to the resulting space charge is about 90 per cent of that of the cloud itself. Illingworth and Wormell explain their recovery data by considering the response of the space charge to the lightning induced field changes.

One of their parameters is T' , the time (s) for the field to recover to half its maximum value, taking the field before the flash to be zero. They found that for events in the first category the field change, E , and the time, T' , were uncorrelated and some of the recoveries were rather irregular. They explained these findings along the lines of Wilson's first proposal but invoked a dissipation mechanism controlled by the clear air conductivity at the height of the cloud (a variable from one cloud to another). One of their chief conclusions is that for flashes closer than about 10 km the effects of the space charge are small but that for flashes at greater distances the recovery of the field measured at the ground can be well explained by the rearrangement of space charge in the vicinity of the storm. Their findings are based on numerical modelling and the fitting of their theoretical predictions to the experimental curves.

Less familiar, perhaps, is the stridulation of certain spiders. Although not so noticeable as the "song" of the cricket, it can, nonetheless, be picked up by the human ear, as Professor H. E. Hinton discovered when he was collecting spiders in 1969 in a Mexican garden. While taking a spiny orb-weaver (*Micrathena gracilis*) from its net, he heard a low pitched buzz which was audible at a distance of about 2 feet. Later, Hinton and his colleague, R. S. Wilson, found that this species possesses a well developed stridulatory organ with a file on the cover of the lung book (the respiratory organ) (*J. Zool.*, **162**, 481; 1970). The scraper consists of three or four stout setae which project dorsally from a small area near the base of the hind femur (see figure). The setae are curved posteriorly and are fairly flexible, and the buzz or hiss is produced when the points of the setae move across the file. The lung book, Hinton and Wilson suggest, may act as a resonance cavity that amplifies the sound.

Seven different types of stridulatory organ have now been distinguished in spiders according to the positions of the files and scrapers. Hinton and Wilson's description of a stridulatory organ in *Micrathena* is the first record of the organ in the family Argiopidae although a similar type of organ—scrapers on the hind legs and files on the covers of the lung books—has been found in a few genera of Erigonidae.

Hinton and Wilson have so far found stridulatory organs in only two species of *Micrathena*. It is interesting that there are specific differences in the spacing of the ridges on the files; in *M. gracilis* the distance from the middle of one ridge to the middle of the next is 8.3–12.0 μm , with most ridges spaced about 11 μm apart; in *M. schreibersi*, on the other hand, the ridges are much closer together (5.0–6.6 μm) and in most areas the spacing is 5.5 μm . In this species therefore the ridges are close enough together to give bright diffraction spectra at incidences near grazing.

The stridulatory organs in spiders could have several possible functions. Courtship is an obvious one but Hinton and Wilson suggest that in *Micrathena* the sound is more likely to function as a warning signal because it is produced when the spider is touched or otherwise disturbed.

DRUGS

Detecting Dependence

from a Correspondent

THE meeting on addictive drugs held at the Institute of Psychiatry, University of London, on January 22, 1971, was sponsored by two different groups of the Biochemical Society (Neurochemistry and Pharmacological Biochemistry) and

turned out to have two quite different themes. One theme was the role of body fluid analysis in the management of drug abuse. The other concerned mechanisms of drug dependence in rodents, particularly the possible relationship between morphine dependence and brain 5-hydroxytryptamine. Each theme is important to the problem of drug addiction in quite a different way—the one in therapy, the other in understanding basic dependence mechanisms—and each was dealt with thoroughly; but the many other aspects of the vast subject covered by the title were omitted.

On the analytical theme, Dr P. H. Connell (Maudsley Hospital, London) pointed out that, whereas most psychiatric patients gave honest answers to questions, compulsive drug takers could not be relied on to do so. The resulting difficulties are greater with younger addicts, who often practise multiple drug abuse. Objective tests are therefore needed to tell the doctor whether the patient is taking (1) unprescribed drugs and, if so, what drugs and how much of each, and (2) the correct doses of drugs prescribed. Without this information, mistakes can be made; for example, a sporadic drug taker may be turned into a regular taker or a recipient may sell the prescribed drug instead of taking it. Because blood is difficult to obtain from "main liners" and blood levels of drugs

are difficult to interpret, reliance is usually placed on urine analysis.

Dr Monamy Buckell (Bethlem Royal and Maudsley Hospitals, London) outlined the difficulties of detecting and estimating drugs in urine. Apart from the heavy load of samples, there was the lack of good methods of estimating cannabis or LSD and the unreliable course of excretion of a drug or its metabolites. Professor A. H. Beckett (Chelsea College, University of London) emphasized the importance of urinary pH in regulating the pattern of excretion, illustrating this with the effect of acidity of urine on excretion of amphetamines and their metabolites and of methadone.

On the rodent theme, Dr H. O. J. Collier and Mr C. Schneider (Miles Laboratories Ltd, Stoke Poges) said that psychic and physical dependence in the rat and physical dependence in the mouse on opiates had only been clearly recognized in recent years, partly because opiate antagonists became available to accentuate withdrawal effects. Because physical dependence develops quickly in these species and because its intensity can be quantitated by counting jumps and other obvious withdrawal effects, the rat and mouse offered a rapid means of obtaining experimental data on physical dependence. These authors described the failure in their laboratory of attempts to confirm the conclusions of Shen, Loh and Way

Identification of Cells producing Interferon

INOCULATIONS of viruses, some other microorganisms or double stranded polyribonucleotides into animals are followed by the appearance of interferon in the circulating plasma. The interferon can be titrated accurately by its capacity to prevent the replication of viruses in cultured cells.

Because interferon may be involved in host protection against infection, it is of interest to identify the cells by which it is produced. Jacqueline and Edouard de Maeyer of the Institut du Radium, Paris, report in next Wednesday's *Nature New Biology* that interferon induction by Newcastle disease virus (NDV) is radiosensitive and its production in mice is depressed by more than 90 per cent after X-radiation of 250 r. The species specificity of interferon enabled the Maeyers to establish that in xenogeneic radiation chimaeras the circulating interferon induced by NDV is of donor origin, from which it can be concluded that the cells synthesizing interferon are derived from blood-forming stem cells.

Both granulocytes and lymphocytes are radiosensitive progeny of such stem cells, and anti-lymphocytic serum (ALS) was used to identify the cells producing interferon. Interferon induction by NDV was found to be markedly depressed by

injections of ALS, which selectively decreased peripheral lymphocyte counts and may also have affected lymphocyte function. This suggests that myxoviruses such as NDV elicit interferon production chiefly by lymphocytes. Indeed, the release of interferon from lymphocytes exposed to viruses, specific antigens such as tuberculin and non-specific mitogens such as phytohaemagglutinin has attracted attention during the past few years—interferon production may be one of the derepressed functions in the lymphocyte exposed to viruses or transformed.

It would be premature, however, to conclude that the lymphocyte is the only cell from which circulating interferon can be derived. Interferon induction by encephalomyocarditis virus, vaccinia virus or polyribonucleoside:polyribocytidylic acid is relatively resistant to radiation or ALS; interferon may be synthesized by macrophages or other cells in these cases. Interferon induction by arboviruses or vesicular stomatitis virus shows intermediate responses to radiation or ALS, less than that on interferon induction by myxoviruses but greater than that on interferon induction by encephalomyocarditis virus. Perhaps some, but not all, of the circulating interferon is synthesized by lymphocytes in these intermediate cases.

that, in the mouse, (1) morphine dependence increases the turnover rate of 5-hydroxytryptamine in the brain and (2) *para*-chlorophenylalanine, which lowers the brain level of 5-hydroxytryptamine, retards the development of dependence. Mr I. Marshall (St Mary's Hospital Medical School, London) also described his failure independently to confirm these two conclusions of Way and his colleagues.

PHOTOSYNTHESIS

Chl *b* Redundant?

by our Plant Physiology Correspondent

THE final nails seem to have been driven into the coffin of the idea that chlorophyll *b* plays a fundamental part in the energy trapping reactions of photosynthesis. This molecule, which at one time seemed to be an integral part of the "z" scheme—the series of interlinked electron movements powered by light which constitute the primary reactions of photosynthesis—has now been relegated to the status of an accessory "spectrum broadening" pigment.

The evidence on which this conclusion is based has been gathered from a number of diverse types of experiments. Chlorophyll *b* has long been implicated in the photosynthesis of green plants and algae on the basis of action spectra measured for photosynthetic enhancement, and it seemed likely that the pigment was an essential component of photosystem 2, the reaction driven by short wavelength light (< 700 nm) which gives rise to free electrons and the evolution of gaseous oxygen. The first crack in this argument came with the demonstration that the pigment complexes for both photosystem 2 and for photosystem 1—the reaction driven by 700 nm light which is ultimately resolved in the production of a source of reducing power—contain a mixture of chlorophyll *a* and chlorophyll *b*. In photosystem 2, the ratio of chlorophyll *a* to chlorophyll *b* is about 2, and the corresponding value for photosystem 1 is approximately 6. Other studies have shown that chlorophyll *b* does not seem to be necessary for oxygen evolution. H. R. Highkin and A. W. Frenkel experimented with a mutant variety of barley which lacked chlorophyll *b*, and showed that in comparison with the wild type there was no decrease in photosynthetic efficiency or evolution of oxygen (*Plant Physiol.*, **37**, 814; 1962).

In blue-green algae it is quite easy to alter the amounts of the various photosynthetic pigments present in the cell by altering the conditions of illumination. B. J. Reger and R. W. Krauss of the University of Maryland have managed to alter the ratio of chlorophyll *a* to chlorophyll *b* in cells of the green algae, *Chlorella*, using this technique (*Plant Physiol.*, **46**, 568; 1970). Cells grown at

6,000 foot candles (high light cells) contained, on a dry weight basis, only one tenth the amount of chlorophyll found in cells grown at 300 foot candles (low light cells). Nevertheless, the high light cells had a greater photosynthetic capacity than the low light cells, a rather strange effect which Reger and Krauss attribute either to a larger number of smaller photosynthetic reaction centres in the high light cells or, alternatively, a small number of normal sized but enzymatically more efficient units.

The normal chlorophyll *a* to chlorophyll *b* ratio for *Chlorella* is about 2. In the high light cells, however, this ratio was increased by the light treatment to 6.2.

In spite of this apparent deficiency in chlorophyll *b* in the photosynthetic system, however, the overall photosynthetic product in the high light culture and the low light culture was not greatly different. Furthermore, some photosynthetic enhancement, which could be attributed to the presence of chlorophyll *b*, could be observed even in the high light cultures.

Reger and Krauss suggest that the level of chlorophyll *b* may reflect a regulatory device which influences the cell's phosphorylation pathways. Their conclusion is based on respiratory data; the rate of respiration in the high light cells was several times greater than that of the low light cells. They suggest therefore that the level of ATP—required for the carbon fixing activities of photosynthesis—may be low in cells where the content of chlorophyll *b* is small, and that this energy must therefore be provided from respiration. They have circumstantial evidence for this; low light cells were bleached and killed by peroxyacetyl nitrate, an inhibitor which specifically blocks the reactions by which ATP is built up photosynthetically in the cells. High light cells, on the other hand, were little affected, suggesting they were deriving their energy from systems other than photosynthesis. Whether this suggestion for the role for chlorophyll *b* is correct or not, it now seems certain that it has little to do with the primary energy trapping reactions.

A Witches' Brew of Neurotoxins

In next Wednesday's *Nature New Biology*, Narahashi, Albuquerque and Deguchi describe the effects of batrachotoxin on the membrane of the perfused squid giant axon. Batrachotoxin, an alkaloid isolated from the skin secretion of the frog *Phyllomedusa aurotaenia*, is the latest in a series of naturally occurring toxins which have been found to possess unique actions on nervous tissues. The compound was first reported to block neuromuscular transmission in the rat and toad by Märki and Witkop (*Experientia*, **19**, 329; 1963). Narahashi *et al.* have now recorded the effects of the toxin on the intracellularly recorded membrane potential of the squid axon. Application of batrachotoxin (1 μ m) to either the outside or the inside of the nerve membrane resulted in a slow and irreversible depolarization of the resting membrane potential. This depolarization was antagonized by the application of tetrodotoxin, which is known to act by reducing the permeability of the nerve membrane to sodium ions (Narahashi *et al.*, *Amer. J. Physiol.*, **198**, 934; 1960). Batrachotoxin also failed to cause a depolarization if sodium ions were absent from the external solution. Narahashi *et al.* conclude that batrachotoxin acts by causing a selective increase in the resting membrane permeability to sodium ions. In spite of this increase in sodium permeability, a further large increase in sodium conductance could still occur when the membrane was electrically depolarized, indicating that the sodium

channels which are available for permeability increase on nerve stimulation remain largely unchanged by batrachotoxin.

Tetrodotoxin, the puffer-fish poison, has already proved its worth as a research tool because of its unique effect in blocking the increase in sodium permeability of excitable membranes normally evoked by stimulation. Batrachotoxin, whose effects seem to be almost the converse of those of tetrodotoxin, also seems likely to be a useful new research tool. These findings may tend to confirm the suspicions of those who feel that research in neurobiology seems to involve magic and witchcraft. Drugs from exotic sources certainly play an important part in this field. For example, the snake venom peptide α -bungarotoxin (Lee *et al.*, *Nature*, **215**, 1177; 1967) seems likely to be important for studies of cholinergic receptors (Changeux *et al.*, *Proc. US Nat. Acad. Sci.*, **67**, 1241; 1970)—supplanting in this role even the more familiar curare, the South American Indian arrow poison. The toxic plant alkaloid bicuculline has also attracted attention because of the report that it represents the first known antagonist of GABA receptors in the nervous system (Curtis *et al.*, *Nature*, **226**, 1222; 1970). If nothing else, these remarkable compounds suggest that nature has so far proved rather more successful than human chemists in devising agents with highly selective actions on the nervous system.

ENZYMES

New Phosphorylations

from our Molecular Biology Correspondent

THE enzymatic phosphorylation of protein side chains is a phenomenon that has long been familiar to enzymologists, but is only now surfacing in molecular biology as an apparently widely distributed, though largely unexplained, control mechanism. The phosphorylation of histones is well known, and recently Allfrey and his colleagues (Teng *et al.*, *Biochem. Biophys. Res. Commun.*, **41**, 690; 1970) have reported that several of the numinous acidic proteins of chromatin rapidly incorporate labelled phosphorus, which turns up as phosphoserine and phosphothreonine residues. A large proportion of the phosphoproteins—the distribution of which has been found to be tissue specific—bind to endogenous DNA, but not to DNA from an unrelated species. These findings clearly offer a new point of application in the study of transcriptional control in animal cells.

Phosphorylation of side chains has also been observed in ribosomal proteins of rabbit reticulocytes by Kabat, who suggested that this process arrests activity *in vivo*. He has now defined the system in more detail (*Biochemistry*, **10**, 197; 1971). Transfer *in vitro* of the labelled γ -phosphorus from ATP to serine and threonine side chains occurs primarily to three components, two of which are identifiable ribosomal proteins. The kinase which catalyses this reaction resides in the ribosome, and is extracted from it by washing with buffers of high ionic strength. The specificity of the kinase is evidently low, for it will efficiently phosphorylate chicken erythrocyte histones. Neither of the two ribosomal phosphoproteins can be easily removed by washing, and one of them especially concentrates in the run-off monomers. The next aim must presumably be to identify the function of this ribosomal protein.

Another, and different, context in which phosphorylation has been suggested to play a part is in the action of DNA-dependent RNA polymerase. Martelo *et al.* (*ibid.*, **9**, 4807; 1970), with a view apparently to explaining the microheterogeneity found in preparations of the *Escherichia coli* enzyme, have tried the effect on it of the AMP-dependent muscle protein kinase, an enzyme that is implicated in regulation of other activities. The introduction of the kinase and ATP into a system containing T4 DNA and the polymerase led to a large enhancement of activity, the more so when 3',5' AMP was also present. A similar effect was caused by an erythrocyte protein kinase, which is presumably the same as Kabat's enzyme. Radioactive phosphorus is transferred from ATP to the polymerase, where it finds its

way into the σ -factor. Indeed, isolated σ -factor can also be phosphorylated with beneficial results on the efficiency of transcription. It is not, of course, by any means obvious that this process is of physiological relevance, but it at least raises the possibility of transcriptional control by selective phosphorylation of one σ -factor, if several of these are in fact present.

It may be remarked in this connexion that a different ATP-dependent modification of *E. coli* RNA polymerase has also just been reported (Chelala, Hirschbein and Torres, *Proc. US Nat. Acad. Sci.*, **68**, 152; 1971). They find that incubation with ATP, magnesium and a cellular fraction results in progressive but reversible inactivation, the reactivation being also dependent on the presence of magnesium. A γ phosphorus label on the ATP is not incorporated into the protein, however, showing that there is no question of phosphorylation. Labels

in the α phosphorus and the carbon of ATP do appear in the protein: the inactivation is therefore apparently caused by adenylation. This is also a phenomenon previously recognized by enzymologists: a thoroughly studied system is the glutamine synthetase of *E. coli*; the function of which is controlled by specific adenylation of tyrosine under the action of another enzyme, adenylyl transferase (see Hennig, Anderson and Ginsburg, *ibid.*, **67**, 1761; 1970). Chelala *et al.* have shown that in RNA polymerase it is not the σ factor that is modified, but rather one or more of the several core proteins.

The relation of phosphorylation to the operation of the glycogen phosphorylase system has exercised a number of illustrious enzymologists; the most recent chapter has now been added by Fischer and his co-workers (*J. Biol. Chem.*, **245**, 6642; 1970, *sqq.*), and this I propose to write about next week.

Phage Under Scrutiny

IN next Monday's *Nature New Biology* a paper by Harrison, Caspar, Camerini-Otero and Franklin most happily and unusually reconciles X-ray diffraction and electron microscopy, and in so doing goes far towards defining the structure in all its complexity. PM2 phage is a marine species, which like some animal viruses contains lipid. The electron micrographs show it to consist of roughly spherical bodies, some 600 Å in diameter, which on closer inspection are seen to be almost certainly icosahedral. There appear to be protruberances at the corners, as in some other icosahedral viruses. The most striking features of PM2, however are the two concentric lines, 40 Å apart, which run round the circumference, with a stain-free intervening region. This is evidently a unit membrane, very similar to that seen in myelin and other systems. Round the outer edge of the membrane there appears to be another layer, consisting of protein.

The X-ray diffraction pattern shows on the one hand a series of regular reflexions at low angles, which correspond to the packing pattern of spheres, and on the other six features at wide angles. One of these is the 3.4 Å reflexion associated with B-form DNA, one is the characteristic diffuse 10 Å ring due to protein, and a broad maximum at about 4.5 Å corresponds to the packing of hydrocarbon chains in the liquid-like phases formed by lipids. This leaves three larger periodicities to be explained. To solve the phase problem Harrison *et al.* use sucrose to change the electron density of the solvent and solvent-penetrable parts of the structure. The electron density map which they obtain shows a deep well at 220 Å, corresponding to the

hydrocarbon sandwich in the bilayer, with peaks due to lipid head groups at 200 and 240 Å. Outside this shell there is a broad peak which diminishes as sucrose is added. This presumably corresponds to an open or fenestrated protein shell through which solvent can percolate. There are four protein species in PM2 phage, and the total amount of protein that would be required to construct an outer shell is such that it must contain at least the preponderant species. This has a molecular weight of 34,000, so that if spherical, each molecule would be 30 Å across. A reflexion corresponding to about this value is in fact present in the X-ray pattern. The structures at the corners presumably account for another protein. From arguments of electron density, volume and composition it emerges that the membrane layer is probably penetrated at intervals by protein.

The electron density in the interior of the particle is too great to be accounted for solely by the DNA, and at least another protein component must therefore be associated with it. An electron density maximum at around 185 Å suggests, that this is at least partly in the form of an internal shell of subunits. The overall diameters of the virus and the lipid bilayer, that emerge from the X-ray diffraction analysis, are in remarkably good agreement with the corresponding dimensions in the electron microscope pictures, showing that there is no significant distortion during the fixing and staining procedures, a result that should give comfort to electron microscopists. Further analysis of the X-ray data has enabled Harrison *et al.* to define features of the structure in considerable quantitative detail.

DIFFERENTIATION

Hybrid Responses

from our Cell Biology Correspondent

LAST week I drew attention to the way in which the technique of somatic cell hybridization is being used to establish linkage groups and map human genes. But apart from the genetic analysis of somatic cells, hybridization is proving to be an extremely useful tool for probing the mechanisms of cellular differentiation. It provides the experimenter with a way of putting two genomes—one differentiated, one undifferentiated—into a single nucleus; one can then ask whether a particular differentiated function is maintained in such a situation. Ephrussi and his colleagues were the first to exploit this approach; they found that inter and intraspecific hybrids between pigment-producing melanoma cells and fibroblasts universally fail to make pigment or dopa oxidase and they suggested that the fibroblasts contain something which prevents the continued production of pigment—in other words, that this particular differentiation is “repressed”. But how generally valid is this conclusion? Only by collecting catalogues of such examples are we likely to find the answer to that central question.

Schneider and Weiss in Ephrussi's laboratory have apparently added one more case to the list as a result of similar experiments involving a different pair of cells (*Proc. US Nat. Acad. Sci.*, **68**, 127; 1971). A line of rat hepatoma cells exhibits several stable differentiated functions; in particular these cells are characterized by high tyrosine aminotransferase activities which are further increased some four to six-fold when the cells are exposed to the steroid, glucocorticoid hormones. Hybrids obtained by fusing these cells with mouse fibroblasts, however, have only low levels of TAT activity, characteristic of fibroblasts, and the hybrid cells are not susceptible to induction by the glucocorticoids. Both rat and murine tyrosine aminotransferases are present in the hybrid cells, which indicates that the structural genes for the enzyme from both parental cells are maintained. The low activity presumably therefore reflects the absence of expression of other genes which regulate the synthesis of the enzyme and are involved in its induction by hormones. Although they have not rigorously excluded every alternative explanation, that is how Schneider and Weiss interpret their data—another example of the repression of a differentiated function in hybrid cells.

Such “repression” is not, however, an inevitable consequence of hybridization, as Schneider and Weiss note, and in the same issue of the *Proceedings* (*ibid.*, 234) Minna, Nelson, Peacock, Glazer and Nirenberg report an impressive example to add to the opposite page of the cata-

logue. Clonal lines of murine neuroblastoma cells possess at least ten properties characteristic of differentiated neurones including electrically excitable membranes and acetylcholine receptors. Do any of these survive in a hybrid obtained by fusing neuroblastoma cells with mouse L cells, a fibroblastic cell which responds to electrical stimulation in a way quite distinct from neurones?

The response of the two sorts of parental cells to depolarizing stimuli can be used as markers. The neuroblastoma cells may be passive or give either an A type or a B type response and the L cells give a third, C type, response. (The precise nature of these responses is not essential to this discussion.) Individual cells in six clonal and one uncloned line of hybrids, derived by fusion of neuroblastoma and L cells, were tested by Minna *et al.* In all these hybrid populations cells giving either A, B or C type responses were detected. The C type

response was less frequent in hybrid populations than in L cell or L × L hybrid-cell populations, but the A and B type responses were at least as frequent in the hybrid populations as in populations of parental neuroblastoma cells.

Clearly at least some differentiated neuronal functions survive in a cell with a neuronal and fibroblastic genome; indeed it has yet to be excluded that neuronal genomes may induce the neuronal differentiation of fibroblastic genomes. Furthermore, hybridization of normal neuroblasts to L cells may be a way of getting into culture those genes which control different types of neuronal differentiation and by correlating the loss of chromosomes from such hybrids with the loss of neuronal functions it may be possible to determine which chromosomes carry those genes. Nirenberg and his colleagues seem to be on the doorstep of the genetic analysis of nerve cell differentiation.

Controlling Cell Wall Extension

ETHYLENE is unique among the naturally occurring plant growth regulators in having a simple molecular construction and being a gas at normal temperatures and pressures. One of its manifold effects on plant growth is to inhibit cell extension in both roots and shoots allowing the cells to fatten by lateral growth. Drs Irene Ridge and Daphne Osborne at the University of Oxford have looked at the dramatic change in the growth pattern of pea shoots caused by ethylene and in particular have considered whether ethylene controls the growth of plant cells by regulating hydroxylation of certain proteins bound to the cell walls. They report their findings in next Wednesday's *Nature New Biology*.

Ridge and Osborne had previously found that application of ethylene to pea apices caused an increase in the amount of hydroxyproline located in the cell walls. Concomitant with this effect was an increase in the levels of peroxidases found in the cytoplasm as well as covalently and ionically bound to the walls.

The cell walls of higher plants are known to contain proteins rich in hydroxyproline which can cross link with wall polysaccharides, notable arabinose, thereby increasing the strength but limiting the extension of the walls. Hitherto, these proteins were considered to be simply structural and to possess no enzymatic action. By fractionation on DEAE cellulose of a cellulase wall extract containing covalently bound peroxidase as a significant proportion of the hydroxyproline, Ridge and Osborne found that the peroxidase and hydroxyproline components show similar fractionation characteristics. This finding

suggests that some of the hydroxyproline-rich wall proteins may be peroxidase.

The peroxidase which is covalently bound to the cell wall differs in two respects from those peroxidases either found in the cytoplasm or just ionically bound to the walls. First, it contains a high concentration of hydroxyproline, and, second, on electrophoresis it separates into isoenzymes which have different properties from the isoenzymes of the other types of peroxidase. From the results of enzyme assays, Ridge and Osborne dismiss the idea that the covalently bound wall peroxidase may act as an additional hydroxylase by converting proline to hydroxyproline in the wall. Ethylene had little effect in the systems tested and they reason that the effect of ethylene on increasing the levels of hydroxyproline in the proteins of pea cell walls is regulated in the cytoplasm, possibly at the level of hydroxylation of specific peptide sequences, the hydroxylated peptides subsequently being transported to the walls.

Ridge and Osborne's article stimulates the question of the possible inter-relationship between ethylene and auxin in the regulation of cell extension. Rayle *et al.* (*Proc. US Nat. Acad. Sci.*, **67**, 1814; 1970) have suggested that the control of rapid cell elongation may be achieved by the degradation of cell wall bonds and it is interesting to speculate that the hydroxyproline-arabinose bond in the cell wall may provide a control site for the action of these hormones. Moreover, another aspect yet to be satisfactorily explained is the control of lateral cell growth in tissues treated with ethylene.

Pangloss and Jeremiah in Science

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Contemporary attitudes towards molecular biology and the environment illustrate well the division between people who feel that science is a force for the good, and people who think that its pursuit can do a great deal of harm. This polarization of attitudes, which has long been evident, recently seems to have reached crisis proportions.

IN one sense there has always been a crisis about science. Certainly the images of science presented to me in a juvenile diet of comics, films and books had a janus-like quality. Professor Branestawm, rather more of a technologist than a scientist, was an absent-minded and pleasantly zany goodie, while Dr Frankenstein was satisfactorily spine chilling and bad. These intensely polarized images of science and technology continue to haunt the mass media. Barbarella meets the Martian Dr Krill, a wizard at genetic engineering. Batman and Robin with the aid of bat science and bat technology continue to save Gotham City.

This dualism in public attitudes to science is thus nothing new, and while the present manifestations of anti-science may well have reached epidemic level, the sense of crisis is, like science itself, endemic in modern society. In 1951 the sociologist Talcott Parsons drew attention to this "ambivalence in public attitudes to science and to the scientist, which are expressed in much irrational and some rational opposition to his role. The obverse of this is that there is a strong non-rational support of the scientist. He is the 'miracle man', who can do incredible things"¹.

Although it is a pity that there have been no long term attitudinal surveys, perhaps starting with Gulliver's Laputa which could have brought definitive light to this issue of the public ambivalence to science, it is possible to take the discussion further by analysing the literature of crisis, its language, content and social basis. Such an analysis is most easily conducted in terms of ideal types, or models, the appositeness of which can be assessed by empirical evidence. Two such types or models suggest themselves. The first, which I describe as the Jeremiad, emerges in a very direct way out of the general literature of crisis. The protagonists of a theory of impending societal doom, as a result either of the intrinsic nature of science and technology themselves or of the social misuse of science and technology, are fairly regularly charged with being Jeremiahs. In spite of this debating style the label commends itself, as the ethical sternness of the Old Testament prophet urging repentance on his degenerate king and people as the only conceivable way of averting disaster bears a more than superficial similarity with the language of the present crisis. Thus "Oh Jerusalem, wash thine heart of wickedness, that thou mayest be saved. How long should thy vain thoughts reside within thee?"².

The prophet seems also to have been aware of the problem of a knowledgeable but morally deficient audience³: "They are wise to do evil, but to do good they have no knowledge".

The second constellation of attitudes and ideologies is represented by Voltaire's Dr Pangloss. As *Candide's* friend and guide through a series of extreme and distressing circumstances, the good doctor was always able to conclude that: "All's for the best in the best of possible worlds". Neither ideal type is particularly flattering. But until we are in a position to sort out the rational and irrational elements of the ambivalence to science, they may serve the better for that.

I shall now examine current attitudes to various fashionable areas of science, notably molecular biology and the environment, in the light of these two models. The novelist Brigid Brophy expresses her newfound hostility to science and technology thus: "Science, which gave promise of delivering mankind from superstition, has itself turned into the most pretentious and bloodiest superstition in history. This will prove to be the tragedy of modern civilisation"⁴. A recent editorial in the magazine *Life* urges immediate repentance: "Air pollution will screen out the sun and make big cities uninhabitable; the fragile biosphere we all live in is becoming poisonous and may cease to support life; . . . plagues threaten"⁵.

Writing in similarly Jeremiad tones, Frank Graham, a journalist, reviewing the battle against pollution, gloomily concludes: "Neither law nor science can restrict any longer the global currents of contamination. In thousands of lakes and rivers the unseen persistent poisons accumulate, tainting whatever life they touch on the journey to the sea"⁶. Clearly Israel has turned its ears against the words of the prophet and "The whole land shall be a desolation"⁷.

Kremlinologist Victor Zorza put his view of the significance of current advances in molecular biology when he wrote of the "Spectre of East-West Genetic Arms Race"⁸. A mother wrote to *The Times* drawing attention to the ethical issues raised by experiments on *in vitro* fertilization⁹: "The ability of scientists to develop the technique of creating life in a test tube. . . . Personally I find the idea of creating life at man's will terrifying. I can only hope that collectively we have sufficient wisdom to outlaw such scientific endeavour"¹⁰. The Duke of Edinburgh expressed concern in more measured plainman tones, speaking at a Conservation Society dinner in November 1969: "We have come to the end of the honeymoon with science and technology".

The social philosopher Jacques Ellul provides a more coherently argued critique of science, an attack on the remorseless rationality of scientific method, "of technique" as he calls it¹¹. He shares the profound disquiet at the new significance of science, with, for example, the *Times* mother. He suspects that, when the geneticists develop the technique of creating life in the laboratory, if they are left with the choice of the type of human to create, they are likely to choose to reproduce their own image. Ellul, being generally unenthusiastic about geneticists, regards this as little less than the doom of humanity, as, beyond the laboratory bench, he sees scientists as capable of little other than intellectual and moral trivia. But in spite of this self-indulgent nightmare, the crux of Ellul's argument is immensely serious. He argues that the increasing application of "technique" to all aspects of social life moves to the point where each technological solution creates another problem in its wake. The car destroys the city with its freeways and exhaust fumes, indestructible plastic containers mount up until we are threatened with submersion below an overflowing mass of rubbish; psychology begins by understanding man and ends by manipulating

him. The arguments of *Brave New World*¹² and *Organisation Man*¹³ are refashioned and presented by Ellul in forbidding cogency.

In bizarre combination the religious conservative Ellul shares with the socialist revolutionary Herbert Marcuse a passionate contempt for the pseudotolerance of what he feels is really an increasingly repressive society. For both, the logical social framework for a society totally committed to the application of scientific method is eventually totalitarian. Personality control, genetic engineering and space travel as part of the programme for the year 2000 are most easily developed within such a framework. For Marcuse the misuse of science and technology is so profound that both have merged with one another and in such a way that the abuse is intrinsic to the activity itself: "The traditional distinction between science and technology becomes questionable. When the most abstract achievements of mathematics and theoretical physics satisfy so adequately the needs of IBM and the Atomic Energy Commission it is true to ask whether such applicability is not inherent in the concepts of science itself. The question cannot be pushed aside by separating pure science from its applications and putting the blame on the latter only, the specific purity of science facilitated the union of construction and destruction, humanity and inhumanity in the progressive mastery of nature"¹⁴.

Nor, for that matter, are any of the other ideologies of the "counter-culture" enamoured of science and technology. Theodore Roszak sees the aggressive scepticism characteristic of both modern science and modern society as having "eroded the traditionally transcendent ends of life . . . under the technocracy we become the most scientific societies; yet, like Kafka's K, men throughout the developed world become more bewildered dependants of inaccessible castles wherein inscrutable technicians conjure with fate"¹⁵. Thus, for Roszak, science, which began as a demythologizing force, by its own logic creates new and darker mysteries.

Emanuel Mesthene, director of IBM's Harvard Program on Technology and Society, seeks to retain the division between technology and what men make of it¹⁶. He therefore sees technology as offering a set of choices and argues that it is what men choose that matters. Thus difficulties in the organization of medical care or urban redevelopment can be resolved by technology through institutional innovation. *Laissez-faire*, suggests the "new left" radical John McDermott¹⁷, has been replaced by Mesthene as *laissez-innover*—the new cure-all. Thus while Mesthene is a sharp critic of the deficiencies in medical care arrangements so that, for example, some have heart transplants while others are denied primary medical care, he believes that these "negative externalities" can and should be resolved by a technological fix. It is this appeal to technology, characteristic of so many Panglossian arguments, which leads McDermott to indict "technology as the opiate of the intellectuals".

Nowhere, of course, is such an opiate more conspicuously present than in the Labour Party's election manifesto of 1964 lauding "the white heat of the scientific revolution" as being synonymous with economic growth, socialism and indeed all human welfare. More recent political statements have recognized the forces of environmentalism, and have moved from such simple euphoria for science to some recognition of the negative externalities. Thus the Labour Government appointed a minister to deal with pollution and the incoming Conservative Government has been equally concerned with the well-being of the environment.

Most of these examples of Panglossian and Jeremiad writings of anti-science have come from the non-scientific side of C. P. Snow's great cultural divide¹⁸. Certainly Leavis in his passionate defence of literarism would have us believe that science and scientism are responsible for culture's ills¹⁹. This outburst resulted, scarcely surprisingly, in an equally impassioned claim for "scientism" by Annan²⁰. But the conception of literarism presented by Leavis and of scientism

by his opponents is limited by a definition of society as something synonymous with the membership of a Cambridge high table. The relevance of either literarism or scientism, and the debate between these polemical campuscules, is thus probably limited.

But irrespective of the validity of the two culture thesis, it is not unreasonable to suppose that non-scientists are much more likely to be Jeremiahs than scientists, and further, that science journalists and science fiction writers, whose *raison d'être* is translating or playing with the world of the one for the other, might be particularly interesting as bridging some sort of cultural gap.

A fairly sharp distinction emerges between American and British science journalists. Where Americans have shown the ability, such as in the case of Rachel Carson, to pioneer Jeremiahs in their own right, their British counterparts have remained content to report what goes on: the facts of science. Thus only when the public debate has begun to be concerned with the social implications of science have they reported these anxieties. For example, Rattray Taylor's *Biological Time Bomb*²¹ was a highly readable speculation concerning the potentialities of science, with enthusiastic sensationalism sometimes overbidding scientific credulity. A subsequent survey of the possibilities and implications of the biological revolution proffered more recently by Gerald Leach²² reflected the change in the times, and offered a more troubled viewpoint, but with few clear conclusions. Nigel Calder is perhaps nearer the American approach in that he has moved from geewhizzery at the wonders of modern science, to his prescription in *Technopolis*²³ for reducing scientific and technocratic power.

In that science fiction writers sell to the public and need not please the scientific establishment, who are in no position to control their access to information, we might anticipate a more free-ranging set of attitudes to science. Indeed, the science fiction writer often assumes that Jeremiah was right and sets his scenario in a post-bomb world, a world in which he demonstrates his own ambivalences to science, by showing the human survivors as hostile to scientific culture. W. M. Miller's *A Canticle for Leibowitz*²⁴, for example, develops this theme and sets out the argument of whether science should be pursued at all. He portrays a world where scientific knowledge has been destroyed either by the bomb, or by the revulsion to science felt by those who survived and followed the holocaust. What is left of science is preserved as religious relics against the day when new scholar scientists can derive renewed meaning from them.

But if the science fiction writers speculate on the limits of the social effects of unbridled science and technology, and are therefore likely to produce images of either heaven or hell for us—anything more moderate might not sell—it is surely the scientists who are most likely to defend science.

Edward Teller, father of the hydrogen bomb, forms an archetypal Pangloss, merging progress and science in one unwrappable package. He speaks of the Plowshare Project, in which nuclear explosives are to be used for building harbours or deep mining, with an unqualified enthusiasm more generally seen in electioneering politicians. Teller cannot entertain even the possibility that there could be hazards of sufficient danger as to render the project non-viable—or even questionable. Any such imputed hazard is, for Teller, merely an aspect of guilt by association with the unfortunately unpopular weaponry programme. Plowshare is progress, and "progress cannot and will not be stopped"²⁵. Although it could be argued that the critique of nuclear testing, when supported by statistics of infant mortality such as those of Sternglass²⁶, might precipitate some Panglossian fervour in reaction, nonetheless relatively few scientists would wish to share Teller's undifferentiated technological imperative. Another physicist, Robert Oppenheimer, speaking of the possibility of making hydrogen bombs, invoked this same spirit—"When you see something technically sweet you go ahead and do it"—a point of view more appropriate to the nineteenth than the twentieth

century in its perception of an inevitable conjuncture of science and progress.

Industrial scientists, however, under attack from the pollution lobby, have had a response on Tellerian lines. Thus at a US Senate sub-committee hearing on the use of pesticides Dr Zaron, a consultant to Shell, testified concerning Rachel Carson's fresh evidence on the ecological dangers of indiscriminate spraying: "Miss Carson is talking about health effects that will take years to occur. In the meantime we would have to cut off food for people around the world. These pedlars of fear are going to feast on the famine of the world—literally"⁶.

Speaking more cautiously to the Biochemical Society in London in December 1969, Dr Philip Handler, president of the US National Academy of Science, discussed the "mounting hysteria exaggerating the problem of the nevertheless quite real threat of varying forms of pollution". He saw the hysteria as emanating from a wider social concern even than that over the war in Vietnam, for it was international in character. He questioned whether it could be the success of science, most of all biochemistry, which had had some part to play in this unrest by "stripping life of its moments of magic". Nonetheless, in spite of his concern with these wider issues, Handler went on to regret that student unrest and the demand for relevance had led to the cutting back of money for basic research. His anxiety narrowed from an analysis of broad social and cultural phenomena, to a plea lest "the urgent should drive out the important" becoming very like a cry not to rock the boat.

The interesting thing about the crisis literature in science is how deeply the sense of crisis—whether as a reformist heresy like that of Luther whose object is to save the church, or as one which, in the eyes of others, will destroy the church—is held by the scientists themselves. This forms the new dimension to the literature of crisis; the scientists themselves are beginning to share the kinds of ambivalences about science which have hitherto predominantly characterized the public, journalists and science fiction writers. The conviction which enabled the editor of *Nature* to write in the 1930s, "My grandfather preached the gospel of Christ, my father preached the gospel of socialism, I preach the gospel of science" has retreated. Paul Ehrlich, the Stanford University population biologist, author of *The Population Timebomb*²⁷, is one of the chief heretics. At a teach-in on pollution he was asked, "If we fail to stem the tide of environmental decay, how long do we have to live?" Ehrlich replied somewhat elliptically but dramatically, "I carry no life insurance and make no plans more than two years ahead".

Nature's contribution to the genetic engineering debate, triggered off in the popular press by the work of Drs Edwards and Steptoe, which received publicity as the "test tube baby", was to diagnose the crisis as a "fuss", partly precipitated by "publicists with scientific doctorates", together with "a side effect of the present concern about pollution that people are prepared almost by reflex to equate the new knowledge with new danger"²⁸.

One event which blew up, or was blown up, around the isolation of much of the *lac* operon of the bacterium *Escherichia coli*, including a single gene²⁹, forms an important benchmark in the crisis debate both within and outside the laboratory. Three years earlier, when Nobel laureate Arthur Kornberg announced the successful *in vitro* synthesis of DNA, the ethical problem of the possibility of "test tube" life was put to him. His reply was that "we can never predict the benefits which flow from advancements in fundamental knowledge. There is no knowledge which cannot be abused, but I hope that an improved knowledge of genetic chemistry will make us better able to cope with hereditary disease. I see no possibility of conflict in a decent society which uses scientific knowledge for human improvement". He left it for us to define, or re-define, a decent society³⁰.

At the end of 1969, however, when Shapiro and Beckwith

released the news of their research, it was coupled with the statement that they feared the knowledge might be used in a socially harmful way³¹. This unaccustomed style of presenting significant research precipitated news headlines ranging from the now imminent threat of genetic engineering to "bacterial bombs". Shapiro and Beckwith denied that they were attacking science and asserted that rather they were concerned to point out that science was developed in a specific social context. The context of America in 1969 was of "a country which makes prodigious use of science and technology to murder the Vietnamese and to poison the environment. . . . The basic control over scientific work and development was in the hands of a few people at the head of large private institutions and at the top of governmental bureaucracies. These people have consistently exploited science for harmful purposes in order to increase their own power"³².

Fellow geneticist and Nobel laureate Salvador Luria entered into the conflict, emphasizing Shapiro's point that "the public's distorted evaluation did not come about because of critics like Shapiro and myself who raise issues about the potential misuse of new technologies in our society. The responsibility rests on the ways which new technologies have in fact been put to mischievous or trivial uses, alongside positive ones, in present day society". Luria went on to cite "nuclear weaponry, the man on the Moon programme, automobile highway boondoggle and trash". He concluded that "we needed more rather than less science, but a social technology and a social science of human living"³³.

Luria is arguing not so much for an increased funding of social science but an increase in the humane element of science. This argument has recently begun to emerge in various ways from the scientists. It reflects itself not only in the rhetoric of environmentalism, but also in the signs that distinguished and creative scientists are turning from their basic research to work on problems of human survival. As John Platt concluded recently, "We have had science for intellectual pleasure, and science for the control of nature, today the whole human experiment may hang on the question of how fast we now press the development of science for survival"³⁴.

It is difficult to assess exactly what is happening, but some light is cast on this process by a lecture given by Nobel laureate Maurice Wilkins at the inaugural meeting of the British Society for Social Responsibility in Science³⁵. Here he spoke of his anxiety that science was no longer attracting and holding the most imaginative people. He feared that if only the unimaginative continued to work in science it would become a juggernaut. Much the same sort of feeling was expressed by the Dainton committee³⁶ in its analysis of the "swing from science". What statisticians have construed, not unreasonably, as a discussion about an actual numerical shortfall in students electing to read science may also read as a reflexion of the crisis in the scientific culture. Whereas in the 1930s, and probably until the dropping of the atom bombs on Nagasaki and Hiroshima in 1945, it was possible for all kinds of scientists to look to science to resolve human ills, many scientists simply do not seem to feel that way any more.

Science is thus passing through an unstructured period of crisis, one both mirrored in the personal crisis of the many minor Shapiros, but also mirrored by the emergence—and re-emergence—of societies for social responsibility in science, in Britain, the United States, Germany and Japan, and similar groupings such as the Union of Concerned Scientists, who ran a one day strike at Massachusetts Institute of Technology to symbolize disquiet at the deformation of science research and teaching through over-dependence on defence contracts. The resolution of this crisis seems to be crystallizing into a search for a different kind of research. Admittedly this kind of interpretation should be realized as only affecting a minority, although possibly a significant minority, of even academic scientists. For most, either the actual or the perceived cut back in basic science spending merely serves to sharpen the rat race. Most of these will continue to observe the "politics

of prudential acquiescence" as Haberer³⁷ has called them, which legitimize a degree of cultural autonomy in science in the face of even a massively hostile state.

In this Haberer gives sociological form to the traditional concept of the neutrality of science. Its significance can be most easily seen under extreme political conditions. Thus the height of Stalin's power and the deformation of socialism, and hence of "socialist biology", ensured the success of Lysenkoist genetics whereby the chronic crisis of Soviet agriculture was offered a solution by a theory of the inheritance of acquired characteristics. The conflict within the Soviet Academy of Sciences, graphically described in Zhores Medvedev's recent book³⁸, could scarcely be regarded as easy acquiescence. For example, for his opposition the Mendelian geneticist Vavilov was exiled and subsequently died in prison. Once the critics were crushed, science began to retreat from its ideologically committed stance. Science became neutralized, for without a sufficiency of cultural autonomy it could not even be competent. While Medvedev's recent incarceration in a psychiatric hospital could be seen as a retreat to this period, it is better interpreted as backlash politics precipitated by his book from those still powerful in genetic institutes who owe their present eminence to a combination of deference to both Stalin and Lysenko; his release also symbolizes their weakened position. At a practical level the possession of powerful friends, notably the physicist Sakharov, the father of the Russian hydrogen bomb, has contributed to his release; but at another level there is a changed cultural perception of science which makes it possible for his friends to be effective. In this new perception the need for a degree of autonomy for science to function at all gives the neutrality of science political recognition.

But the degree of cultural autonomy is best preserved when the goals and activities of science are seen as acquiescent to those of the state. Thus Heisenberg is reported to have personally lobbied Hitler in order to take the race issue out of physics, because without Einsteinian physics German physics could not proceed. But Heisenberg's concern to protect physics did not extend to the protection of his fellow but Jewish physicists, nor did it reflect any anxiety at the use to which physics might be put by the Nazi regime³⁹.

As the philosopher Ernest Gellner observed, "it is not so much that science is morally neutral, as that it is morally meaningless"³⁹. We are seeing an attempt to make science morally meaningful. Science is no longer to be assumed to happen in a social vacuum; the traditional belief that it was driven solely by its own inner logic is challenged. The outer logic of contemporary society's choices which constrain the inner logic is being articulated. The inner logic, that is the sequence of ideas, hypotheses, experiments and theories which order the progress of science, is itself constrained by the science which has preceded it. Thus it is difficult to see how the new biology, with the cracking of the genetic code and the emergence of biochemistry as a unifying science with explanatory powers over many areas of biology which had traditionally appeared as diverse, could have arisen except on the basis of the prior concepts of nineteenth century organic chemistry and the gene models introduced by Mendel-Müller genetics in the early twentieth century.

Nonetheless, although the inner logic of science is a necessary condition for a particular advance, this is not the same as a sufficient condition. Resources must also be available. But equally the mere willing of resources will not automatically promote significant advance. Nor even if the knowledge structure and financial support are present does this ensure success; as we have seen and are presently seeing, certain sorts of question become unaskable in specific social contexts. But even when an element in the internal logic or external logic of science is missing, it does not follow that the scientific activity will stop in the numerical sense of scientists doing science, but in a qualitative sense of the kind they find it

possible to do. Science is increasingly an occupation and decreasingly a vocation⁴⁰.

While in the first phase of the crisis a kind of Luddism seemed to be emerging, in the second phase there is something like a new science developing which attempts to preserve the creative delight of science yet couple it to ends which can less easily be distorted. In this first phase no form of science had a positive place in the debate about the counter culture, only negatively, in the attack on the massive misuse of science in Vietnam. Now there are signs that science itself is being radicalized. Teaching programmes are being developed to elucidate the social implications of science, science courses are set in a social context, increasingly scientists are "turning on" to the human environment. The question I began by discussing in terms of Panglossian and Jeremiad attitudes to science now moves explicitly into the political arena, raises all the issues about the relationship of scientific knowledge to the system that pays for it and within which it is done, to the processes of social change.

Nature's leading article for the last general election was called "Science is Neutral this Time"⁴¹, a title which, although overtly addressed to transient electioneering issues, carried in it undertones of another message. That to stay "neutral", to stay in the politics of prudential acquiescence, to stay in the world of Dr Pangloss, is going to be rather more difficult "next time".

¹ Parsons, T., *The Social System*, 338 (Free Press of Glencoe, New York, 1959).

² Jeremiah, *Holy Bible*, ch. 4, verse 14.

³ Jeremiah, *Holy Bible*, ch. 4, verse 22.

⁴ Brophy, B., *The Listener*, November 27 (1969).

⁵ *Life*, March 6 (1970).

⁶ Graham, F., *Since Silent Spring* (Hamish Hamilton, 1970).

⁷ Jeremiah, *Holy Bible*, ch. 4, verse 27.

⁸ Zorza, V., *Guardian*, December 3 (1969).

⁹ Edwards, R. G., Barister, B. D., and Steptoe, P. C., *Nature*, **221**, 632 (1969).

¹⁰ LePatourel, B., *Times*, February 24 (1969).

¹¹ Ellul, J., *The Technological Society*, 432 (Cape, 1965).

¹² Huxley, A., *Brave New World* (Chatto and Windus, 1947).

¹³ Whyk, W. J., *The Organisation Man* (Cape, 1957).

¹⁴ Marcuse, H., in *Science as Culture* (edit. by Holton, G.), 232 (Houghton Mifflin, 1965).

¹⁵ Roszak, T., *The Counter Culture*, 13 (Faber and Faber, 1970).

¹⁶ Mesthene, E., *Program on Technology and Society, Fourth Annual Report*, 1967-8 (Harvard University Press).

¹⁷ McDermott, J., *New York Review of Books*, July 31 (1969).

¹⁸ Snow, C. P., *The Two Cultures and the Scientific Revolution* (Cambridge University Press, 1959).

¹⁹ Leavis, F. R., *Times Literary Supplement*, 3556, p. 441, April 23 (1970).

²⁰ Annan, N., *Times Literary Supplement*, April 30 (1970).

²¹ Rattray Taylor, G., *The Biological Time Bomb* (New York, 1968).

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²³ Calder, N., *Technopolis* (Macgibbon and Kee, 1969).

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²⁵ Teller, E., *New Scientist*, 348, February (1970).

²⁶ Boffey, P., *Science*, **166**, 195 (1969).

²⁷ Ehrlich, P., *The Population Timebomb* (Ballantine Books, 1968).

²⁸ "Premature Birth of Test Tube Baby", *Nature*, **225**, 886 (1970).

²⁹ Shapiro, J., Machattie, L., Eron, L., Ihler, G., Ippen, K., and Beckwith, J., *Nature*, **224**, 768 (1969).

³⁰ Cooke, A., *Guardian*, December 17 (1967).

³¹ Glassman, J. R., *Science*, **167**, 963 (1970).

³² Shapiro, J., Eron, L., and Beckwith, J., *Nature*, **224**, 1337 (1969).

³³ Luria, S., *Nature*, **225**, 301 (1970).

³⁴ Platt, J., *Science*, **166**, 1116 (1969).

³⁵ *Brit. Soc. Social Resp. Sci. Newsheet*, No. 1 (April 1969).

³⁶ *Enquiry into the Flow of Candidates, in Science and Technology, into Higher Education* (Cmd. 3541, HMSO, 1968).

³⁷ Haberer, J., *Politics and the Community of Science* (Van Nostrand Reinhold, 1969).

³⁸ Medvedev, Z., *The Rise and Fall of T. D. Lysenko* (Columbia University Press, 1969).

³⁹ Gellner, E., *Thought and Change* (Weidenfeld and Nicolson, 1964).

⁴⁰ Cotgrove, S., and Box, S., *Science, Industry and Society* (Allen and Unwin, 1970).

⁴¹ *Nature*, **226**, 677 (1970).

Decimalization under Cromwell

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Although Britain is only now adopting a decimal currency, the idea was first mooted three hundred years ago.

IN the climate of speculation aimed at solving the economic crisis provoked by the civil wars, decimal coinage was proposed by the little known Oxford scholar, Robert Wood (c. 1622-85) in a short tract entitled "Ten to One". This is undated, but it was probably composed in 1655 or 1656. The single manuscript copy which has survived is complete and in a form ready for circulation or publication. Its full title is "TEN TO ONE. A short and ready way For the extraordinary Facilitation & Dispatch of RECKONINGS. By means of Two or Three NEW DECIMAL COINES much desired in order to the Publick Good by R. W."¹

This prophetic tract and its author have been completely overlooked by commentators on seventeenth century economics, Wood being known only as a minor mathematician². However, he attained more celebrity than is generally recognized during the interregnum. After attending Eton, where he was a school friend of Robert Boyle, Wood entered Merton College, Oxford, graduating in 1647. At this time wholesale expulsion of Royalist academics gave widespread opportunities for preferment at Oxford, Wood being intruded as fellow of Lincoln College in 1650. He followed the course of many able mathematicians, studying under William Oughtred, rector of Aldbury near Guildford. He was obviously successful, being entrusted with the English translation of Oughtred's famous textbook, *Clavis Mathematica*³.

Wood's association with Oughtred probably provides the key to his advocacy of decimalization. Oughtred's book began with an introduction to decimal calculation and decimal fractions, which were recognized as having advantages over the sexagesimal method used by astronomers. *Clavis Mathematica* also provided an introduction to decimal logarithms, based on the recent work of Napier and Briggs. Oughtred had been a pupil of Briggs and his first publication was perhaps *An Appendix to the Logarithms* in Briggs's edition of Wright's translation of Napier's famous *Mirifici logarithmorum canonis descriptio*. During Wood's youth, the recognition of the value of logarithms and the calculation of comprehensive tables was one of the great achievements of European mathematics. Wood recognized that currency decimalization would assist the application of logarithms to monetary calculations. "The way here proposed, it carries with it others [advantages] no less considerable from the helpe of Artificial Numbers or Logarithmes (the most rare & absolute Invention for the dispatch of Arithmetical Operations that ever was or perhaps ever will be discovered) which are applicable only to this decimal way" ("Ten to One", page 5).

The final stimulus to Wood's proposal came from his involvement with the circle of philanthropists and economic reformers advising Cromwell. This group, headed by Samuel Hartlib, were convinced that technological innovation could solve many of the nation's economic problems. The informal economic debate organized by Hartlib involved Benjamin Worsley, Secretary of the Commonwealth Council of Trade, the influential merchant Henry Robinson, the pamphleteer William Potter and the young William Petty. Wood, already a friend of Petty at Oxford, joined this circle. His first letter

to Hartlib, dated 1-11 March 1656, enclosed the pamphlet "Ten to One".

This proposal was addressed to Cromwell and his advisers, arguing that monetary reform should rest on the principles of mathematics. Wood optimistically suggested that little cost would be involved in the decimalization of currency. The prevalent duodecimal system based on shillings, pence and farthings (20th, 240th and 960th parts of a pound) was not only at odds with general methods of computation, but it also caused "distraction of the mind, and consequently pronenes to mistake". This prevented merchants readily casting accounts or checking for errors made by their employees. With the adoption of decimalization the merchants would save labour and find accountancy "turn'd to a delightfull recreation" ("Ten to One", page 3).

The pound was to remain the standard for currency. Three inferior coins were proposed; "tenths" equivalent to two shillings; "hunds" worth about two pence; finally "thous" valued at a little less than a farthing. The pound would thus be divided into ten tenths, 100 hunds and 1,000 thous, foreshadowing the most popular Victorian proposal and the one now adopted, with the elimination of the lowest value ("Ten to One", page 3).

Wood was sure that this system would be readily comprehended by the educated classes and would not prove too difficult for the rest. Any difficulties were outweighed by this rational system which would ultimately yield economic advantages. The first practical step urged by Wood was the introduction of tenths, bearing the "image and superscription" of Cromwell. Coins were indeed introduced bearing Cromwell's image, but no two-shilling piece was produced until 1849, when the florin appeared. The hund has not materialized until February 1971, while the thou has been rendered unnecessary by inflation.

While there is no evidence about official reaction to Wood's proposals, probably due to preoccupation with more immediate and serious economic problems, "Ten to One" was distributed among prominent parliamentarians and economists. In 1657, Wood reported to Hartlib, in a letter of August 1, that his scheme was accepted by the "Mathematical Professors and others" at Oxford, as well as Petty and Miles Symner in Dublin⁴. Thus his idea was accepted by the most influential scientific circle of the protectorate, the "Oxford group", which included Wallis, Ward, Wilkins and Wren. This was at least sufficient to keep the decimalization proposal alive. Thus two brief decimalization schemes were proposed during the vigorous debate on monetary theory at the end of the century. Significantly one came from Wren, who may have remembered Wood's original suggestion⁵. Perhaps the only immediate outcome of "Ten to One" and Wood's other economic speculations was a successful career in Cromwell's civil service.

¹ Hartlib Papers, manuscripts in Sheffield University Library, Bundle XXVII, section 20.

² Wood, A., *Alumni Oxoniensis* (edit. by Bliss, P.), 4, 167 (1820); Aubrey, J., *Brief Lives* (edit. by Clark, A.), 1, 120, 290; 2, 113, 141 (Oxford, 1898); Mathews, A. G., *Calamy Revised*, 542 (Oxford, 1934); *Dictionary of National Biography*, 62, 372; Taylor, E. G. R., *The Mathematical Practitioners of Tudor and Stuart England*, 226, 347 (Cambridge University Press, 1954).

³ *The Key of Mathematicks New Forged and Filed* (London, 1647).

⁴ Hartlib Papers, manuscripts in Sheffield University Library, Bundle XXXIII, section 1.

⁵ Horsefield, J. K., *British Monetary Experiments 1650-1710*, 292, 325 (Harvard University Press, 1960); *Sir Christopher Wren's Proposal* [1695].

* The Hartlib Papers are quoted by permission of their owner, Lord Delamere.

Evidence for the Possible Existence of a Superheavy Element with Atomic Number 112

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Evidence is presented for the possible existence of a superheavy element produced by secondary reactions in a tungsten target bombarded by 24 GeV protons.

THE possible existence of long lived superheavy nuclei due to the formation of a closed shell of protons around $Z=114$ and a closed shell of neutrons around $N=184$ has been the subject of many¹⁻⁶ theoretical papers. Predictions of the alpha, beta and spontaneous fission stability of nuclei in this mass region have been made⁷⁻¹¹ and half-lives of up to 10^8 years for some superheavy nuclei have been predicted. It has been argued that perhaps the best method to produce these new elements would be the bombardment of a heavy element target with very heavy ions, at energies above the Coulomb barrier which exists between the target nuclei and the projectile (see ref. 12, for instance). Heavy incident ions are required to give the large neutron excess of the final nucleus. For some particular cases cross-sections of the order of about $10 \mu\text{barn}$ have been estimated for the production of superheavy nuclei (S. A. Karamyan and Yu. Ts. Oganessian, unpublished).

This article describes an attempt to produce superheavy elements by secondary reactions using beams of very high energy protons. A very high recoil energy may be achieved when such a proton is elastically scattering at a large angle from a heavy nucleus. For instance, if a 24 GeV proton is elastically scattered from a W nucleus at angles between 45° and 180° to the incident beam, the recoil energy would be between 1.0 and 5.6 GeV respectively. A (p,n) or (p,p') reaction, or alternatively reactions which involve a higher mass for the emitted particles such as (p,d) (p, α) and so on, could also produce high energy recoil nuclei. These recoil energies can then exceed the Coulomb barrier of about 1.0 GeV which exists between two W nuclei. The superheavy elements could then perhaps be produced as a result of the interactions of the recoiling nucleus with another heavy nucleus in the target either due to an asymmetric fission reaction (Karamyan and Oganessian, unpublished) or perhaps through an unusual type of transfer reaction.

Neither of the processes for the production of fast recoil nuclei mentioned here has been studied for the range of angles of interest in our experiment. Measurements^{13,14} at 19.2 and 3 GeV for the elastic scattering of protons at small angles show a sharp decrease in the cross-sections with increasing angle and it is difficult to estimate from these measurements the

cross-sections at much larger angles. On the other hand it may be argued that the cross-sections for the (p,n) and other inelastic reactions mentioned will decrease much less rapidly with angle. If we assume a total cross-section of about $1 \mu\text{barn}$ for the production of a "beam" of high energy heavy ions from 24 GeV protons (due to all the processes mentioned here and perhaps others) and we use the predicted cross-section of about $10 \mu\text{barn}$ for the induced fission reaction (Karamyan and Oganessian, unpublished) we could expect a production yield of about 10^3 superheavy atoms in a typical target of 120 g cm^{-2} of tungsten bombarded with about 10^{18} protons of 24 GeV energy. This may be a rather conservative estimate as the predictions of Karamyan and Oganessian probably underestimate the production cross-section.

Because superheavy nuclei will decay to daughter nuclei of unknown properties, we cannot use the identification of the daughter nuclei to fix the Z and A of the parent decay. Also X-ray transitions cannot be used to identify the Z of the decaying nucleus because of the very small number of nuclei likely to be produced. It is therefore necessary to rely on the predictions¹⁵ that elements 110, 111, 112, 113 and 114 will be the chemical homologues of Pt, Au, Hg, Tl and Pb respectively, both for the identification of the atomic number of the element and for their separation from the 10^{23} atoms of other elements also present in the bombarded target.

Experiments have been carried out to separate actinides from the same targets as were used in the present work. The results so far indicate that significant alpha activities have been observed from elements which closely follow the known chemistry of actinides. It has not so far been possible, however, to identify with absolute certainty any specific alpha activity with known actinide isotopes. These results suggest that elements with $Z > 94$ may have been formed and it therefore seems possible that if superheavy elements ($Z > 110$) exist they could be formed in the same way.

In this article we discuss measurements made on the Hg sources. Measurement on other sources are in progress. Both spontaneous fission and alpha activity have been observed in Hg sources prepared from the bombarded tungsten target. We believe that a possible explanation of these results is the presence of one or more isotopes of the element 112. Although the best evidence is that based on the observation of spontaneous fission we shall discuss the alpha spectra first as some of these results are used in the interpretation of the fission data.

Chemical Separation

Two cylindrical tungsten targets, each 120 g cm^{-2} thick and consisting of 33 g of 99.95% tungsten, were obtained after irradiation by the CERN proton-synchrotron. The first was bombarded by about 2×10^{18} protons of 24 GeV energy over a

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period of about a year and was three to four months old when it came into our possession. The second target was irradiated with about 7×10^{17} protons of the same energy over a period of about four months and we started the chemical separation a few days after the irradiation. We will refer to the first target as W1 and to the second as W2.

The targets were dissolved anodically at room temperature in an alkaline medium. Precautions were taken to avoid any loss of volatile products. The dissolution was carried out in the presence of 40 μg each of Os, Pt, Au, Hg, Tl and Pb as carriers. In addition, 1 mg quantities of Te, I, Ba, La, Zr, Ta and Sb were added as hold-back carriers for subsequent chemical operations. The bulk of the radioactive products, including the elements Os to Pb, were separated from the dissolved tungsten target by coprecipitation onto MnO_2 . It was assumed that the eka-elements would follow their homologues throughout these chemical separations.

The MnO_2 was dissolved in $\text{HCl}/\text{H}_2\text{O}_2$ (Au and Tl were extracted into ether at this point). The solution was adjusted to 1.5 M HCl and passed through a De-acidite FF anion exchange column. The elements Pt, Hg and Pb were retained on the column, and the actinides, among other elements, were not absorbed. The Hg was eluted with 0.01 M HCl/thiourea. The complex was decomposed with HNO_3 ; the solution was adjusted to pH 2.5 with ammonia and the Hg extracted into CCl_4 as the dithizonate in the presence of EDTA as a masking agent. The Hg was back-extracted with dilute HCl and the dithizone extraction repeated as before. This separation procedure is specific for mercury.

Extrapolation of the periodic properties of the elements suggests that compounds of eka-mercury and the element itself may be more volatile than mercury. Sources were therefore prepared by vacuum evaporation of HNO_3 solutions at room temperature to minimize volatilization of the eka-mercury. One of the chief difficulties in preparing the sources was to reduce their weight in order to make them suitable for alpha particle and fission fragment counting. This difficulty is severe with Hg because it is not possible to heat the sources in order to remove any organic material present. Both sources were about 2 mg cm^{-2} thick.

Alpha Spectroscopy

Silicon surface-barrier detectors were used to detect the α particles and extreme precautions were taken to avoid contamination of the detectors themselves. New detectors were used for the counting of all sources. Energy calibrations were made using an identical type of detector and it was shown that changing one detector for another of exactly similar type changed the calibration by less than 0.2%. The calibration of the whole system of detector, amplifier and so on was observed to be stable to better than 0.5% over a period of several months. As confirmation that contamination of the apparatus was negligible no counts were observed over a period of ten days with a blank platinum sheet in place of a source. The Hg sources were cooled to liquid nitrogen temperature to prevent the possible evaporation of the eka-mercury which was under vacuum for long periods.

Fig. 1 shows alpha particle spectra which were measured with the Hg sources. The source which was obtained from the second W target was counted twice, for 406 h and for 236 h and the results are shown in Figs. 1a and b respectively. The average time difference between these two measurements was about 24 days. Fig. 1c shows an alpha particle spectrum obtained with the Hg source from the first W target. This spectrum was collected over a period of 280 h. In all these spectra one can see a peak at $6.73^{+0.150}_{-0.050}$ MeV. (The larger uncertainty towards the higher energy is due to the thickness of the sources.) The total number of counts under the 6.73 MeV peak at 12, 7 and 12 in Figs. 1a, b and c respectively. A comparison of Fig. 1a and b shows that within the statistical accuracy this peak did not decay over a period of 24 days.

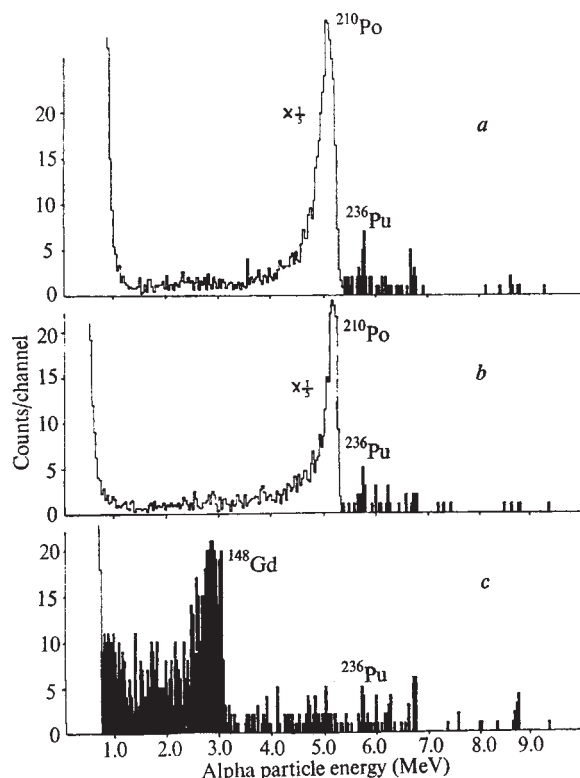


Fig. 1 Alpha particle spectra measured with the Hg sources. a and b were taken over periods of 406 and 236 h respectively with the source obtained from the second tungsten target. c was obtained over a period of 280 h with the source from the first tungsten target.

In the range of energies from 6.67 to 7.00 MeV there are more than forty known alpha groups which could in principle be candidates for producing the observed peak at 6.73 MeV which did not decay over a period of 24 days. Most of the alpha groups, however, are due to the decay of short lived isotopes which, because they do not have a parent of half-life greater than 10 days, could not be responsible for the observed peak. In the few other cases they can be excluded as candidates for producing this peak because, if they were present, other pronounced alpha groups, which we did not see, should also be observed. Perhaps the most problematic case is the 6.78 MeV group of ^{216}Po which exists in the decay chain of ^{228}Th (half-life of 1.91 years). In the decay of ^{228}Th , however, there are other groups of alpha particles at 5.68 and 6.29 MeV of equal intensity and at 5.43 and 8.78 MeV of comparable (70%) intensity to the 6.78 MeV group. In all the spectra shown in Fig. 1, the peak at 6.73 MeV is very pronounced; it is the largest high energy group in Fig. 1c and in Figs. 1a and b only the groups at 5.14 MeV and 5.73 MeV which we believe are due to ^{210}Po and ^{236}Pu are larger than the 6.73 MeV peak. In Figs. 1b and c we do not see a pronounced peak at 5.43 MeV and in Figs. 1a and c we do not see a strong group at 5.68 MeV. (The few counts that exist in this region could also be due to the tail of the ^{236}Pu group.) On statistical arguments, it therefore seems very unlikely that the observed 6.73 MeV peak is principally due to thorium contamination.

The peak at 5.14 MeV seen in Figs. 1a and b with an upper edge at 5.30 MeV is probably due to the 5.31 MeV alpha particle from the decay of ^{210}Po . The difference in the energy of the peak and the possible true alpha energy is a reflexion of the thickness of the source. Possibly due to slight differences in the chemical separation techniques, this peak is not observed in the spectrum, Fig. 1c, from the W1 source. The peak at 5.73 MeV in Figs. 1a and b may be due to the 5.75 MeV decay of ^{236}Pu . We do not at present have any clear explanation for the peak at 8.78 MeV; as we have shown, it is unlikely to be due

to the decay chain of ^{228}Th . The peak at 3.1 MeV in Fig. 1c is probably due to the decay of ^{148}Gd .

It might be argued that the peak at 6.73 MeV could be due to contamination by some unknown actinide which is also produced in the original target. The results of measurements on the actinides will be given later and here we summarize only the important features which are relevant to the present discussion.

(a) Actinides seem to be produced in the tungsten target.

(b) The most pronounced group of alpha particles observed in the spectra is at 3.18 MeV due to the decay of ^{148}Gd which is expected to be chemically separated with any actinides.

(c) A group of 6.75 MeV which appears in the spectra of most of the actinide sources has a total intensity of about 1/250 of the intensity of the 3.18 MeV group.

It is reasonable to assume that the decontamination factor for gadolinium would be similar to that for the transamericium actinides in the Hg chemical separation procedure. On this basis, and from the results of the actinide measurements mentioned here, one concludes that only two counts in the 6.73 MeV group observed in all the spectra from the Hg source may be due to the 6.75 MeV group of the actinides.

If, however, the 6.75 MeV group that appeared in the spectra from the actinide sources is due to decay of an unknown daughter of an actinide, the comparison with the intensity of the 3.18 MeV peak is no longer valid. The contamination in this case may be larger.

Finally, we should comment that it seems unlikely, on the basis of half-life, that the alpha group is due to any unknown decay of mercury even from an isomer state.

Observation of Spontaneous Fission

As evidence for a superheavy element based solely on alpha decay might not be considered convincing, we also searched for spontaneous fission events occurring in the Hg source using polycarbonate films (*Makrofol KG*, supplied by Bayer Chemicals Ltd) 1.2 mg cm⁻² thick to detect the fission fragments¹⁶. Holes due to fission fragments were observed in several cases. It was confirmed that the spatial position of the holes corresponded with the area covered by the source. Measurements were started about two months after the separation of the mercury source from the W2 tungsten target. The results can be summarized as follows.

(1) A foil which was kept for seven days above the Hg source produced from the second W target (W2) had eight holes in it.

(2) Another foil held four days later for a period of fourteen days above the same source had thirty-four holes in it.

(3) Following attempts to reduce the thickness of the W2 source a third foil held above it for a period of eight days had twenty-eight holes.

(4) A fourth foil held above the same source thirteen days later for eight days had twenty-three holes.

(5) Four similar but unirradiated foils processed in an identical way had no holes.

(6) Eight foils kept for twelve days above Pt sheets identical to those used as backings for the Hg sources had an average less than one hole in each foil.

On the basis of these results we would conclude that approximately 93 fission fragments from spontaneous fission in the Hg source from target W2 have been observed over a period of 37 days. (Preliminary measurements on a mercury source from a third tungsten target very recently received also give evidence for the observation of spontaneous fission.)

Spontaneous fission is a rare mode of decay and is confined almost entirely to the elements above uranium. But we do not believe that the spontaneous fission events observed in the mercury source can be ascribed to contamination. Certainly they are not due to the well known spontaneous fission of ^{252}Cf (2.5 years) as we do not see the associated alpha group at 6.1 MeV. Estimates of the decontamination factors from various elements for the Hg sources made by comparing

alpha groups indicate that the fission events observed with the Hg source are unlikely to be due to any contamination. They are also unlikely to be due to the fission of a mercury isotope or of a metastable state as events of this type have not previously been reported in this mass region.

It is of some interest that so far we have no evidence for spontaneous fission occurring in the Pt, Au and Tl sources prepared at the same time as the Hg source. This indicates that the fission is specific to the Hg source and may be due to the decay of element 112 or one of its daughter products. For a crude estimate of the lifetime we assume that a maximum of 10^6 atoms of the isotope responsible were formed in the target. The rate of decay then implies an upper limit to the half-life of approximately 500 years. An estimated lower limit for the longest lived member in the decay chain producing the spontaneous fission is one month. No fission events were observed with the W1 source. This may be due either to the effects of source thickness or because it was about a year older than the W2 source when these measurements were made.

Clear evidence that the fission is due to a superheavy element would be given by a measurement of the fission energy spectrum. Unfortunately this is not at present possible because of the thickness of the source.

Comparison with Predictions for Element 112

It seems reasonable to conclude that the alpha group at $6.73^{+0.15}_{-0.01}$ MeV and the fission fragment which we have observed may be due to decay of one or more isotopes of element 112. We have shown already that they seem unlikely to be due to the actinides or any other known activity. On the other hand the chemical properties of element 112 are predicted¹⁵ to be similar to those of Hg and to be different from those of the elements from 104 to 111.

Our results give a disintegration energy of about 6.82 MeV assuming 290 for the mass of the recoil nucleus. This fits well with the predictions for beta-stable isotopes of element 112 by Nilsson *et al.*^{8,10} and Muzychka¹¹ which lie in the range 6.5 to 7.0 MeV, and not so well with that by Lightman and Gerrace⁹ of 7.5 MeV. The predicted half-lives^{8,10} lie in the range 10^3 to 10^4 years. Then if we assume a partial half-life of about 10^3 years for the isotope of element 112 which decays by alpha particles of 6.73 MeV energy, the number of atoms of this isotope existing in our sources is estimated to be 10^5 – 10^6 .

Spontaneous fission half-lives in the range 10^6 to 10^{13} years were predicted by Nilsson *et al.*^{8,10} for beta-stable isotopes of element 112. Our results would indicate a much shorter lifetime, but still within the uncertainty of the predictions. It may also be that the observed fission events are due to a lighter isotope of element 112 where the spontaneous fission half-life is predicted to be shorter than the alpha half-life.

To summarize, we believe that we may have observed the production of element 112 by secondary reactions in tungsten targets irradiated by 24 GeV protons. We have observed spontaneous fission (which is specific to the mercury source) and which is unlikely to be due to any contamination by actinides or due to fission from some unknown isomeric state of mercury. We have also observed an alpha-decay at 6.73 MeV energy which, although with less certainty, is unlikely to be due to any contaminant and whose energy agrees with several sets of predictions for element 112.

Many people in different laboratories have helped us and we are grateful to them. In particular we thank P. H. Standley and Ch. Steinbach from CERN for providing us with the irradiated tungsten targets, Mrs K. M. Glover and F. J. G. Rogers from AERE for help at many stages, G. B. Stapleton for making available the facilities of the radiochemistry laboratory, and J. G. Cuninghame, Drs A. M. Friedman, G. Manning, G. H. Stafford and Professors K. Bagnall, W. D. Allen and S. G. Cohen for discussions.

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Protein Polymorphism as a Phase of Molecular Evolution

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It is proposed that random genetic drift of neutral mutations in finite populations can account for observed protein polymorphisms.

SINCE one of us¹ put forward the theory that the chief cause of molecular evolution is the random fixation of selectively neutral mutants, some have supported the theory²⁻⁵ and others have criticized it⁶⁻⁸.

Rate of Evolution

Probably the strongest evidence for the theory is the remarkable uniformity for each protein molecule in the rate of mutant substitutions in the course of evolution. This is particularly evident in the evolutionary changes of haemoglobins⁵, where, for example, the number of amino-acid substitutions is about the same in the line leading to man as in that leading to the carp from their common ancestor. Similar constancy is found on the whole for cytochrome C, although the rate is different from that of the haemoglobins. The observed rate of amino-acid substitution for the haemoglobins is very near to one Pauling (10⁻⁹/amino-acid site/yr) over all vertebrate lines⁵. The rate for cytochrome C is roughly 0.3, while the average rate for several proteins is about 1.6 times this figure².

If we define the rate, k , of mutant substitution in evolution as the long term average of the number of mutants that are substituted in the population at a cistron per unit time (year, generation and so on), then under the neutral mutation-random drift theory, we have a simple formula

$$k = u \quad (1)$$

where u is the mutation rate per gamete for neutral mutants per unit time at this locus. Note that this rate k is different from the rate at which an individual mutant increases its frequency within a population. The latter depends on effective population size.

The uniformity of the rate of mutant substitution per year for a given protein may be explained by assuming constancy of neutral mutation rate per year over diverse lines. Moreover,

the difference of the evolutionary rates among different molecules can be explained by assuming that the different fraction of mutants is neutral depending on the functional requirement of the molecules.

On the other hand, it can be shown that if the mutant substitution is carried out principally by natural selection

$$k = 4N_e s_1 u \quad (2)$$

where N_e is the effective population number of the species, s_1 is the selective advantage of the mutant and u is the rate at which the advantageous mutants are produced per gamete per unit time⁹. In this case we must assume that in the course of evolution three parameters N_e , s_1 and u are adjusted in such a way that their product remains constant per year over diverse lines. The mere assumption of constancy in the "internal environment" is, however, far from being satisfactory to explain such uniformity of evolutionary rate. In our example of carp-human divergence, we must assume that $N_e s_1 u$ is kept constant in two lines which have been separate for some 400 million years in spite of the fact that the evolutionary rates at the phenotypic level (likely to be governed by natural selection) are so different.

Polymorphism in Sub-populations

Kimura¹ also suggested that the widespread enzyme polymorphisms in *Drosophila*¹⁰ and man¹¹ as detected by electrophoresis are selectively neutral and that the high level of heterozygosity at such loci can be explained by assuming that most mutations at these cistrons are neutral. This suggestion, however, has been much criticized¹²⁻¹⁴. One of the chief objections is that the same alleles are found in similar frequencies among different sub-populations of a species and that some kind of balancing selection must therefore be involved.

Robertson¹⁵ suggested that if a large fraction of mutations at a locus is selectively neutral, we find either very many alleles segregating in large populations, or a small number of different set of alleles in different isolated small populations. He considered that because neither of these alternatives is found, most polymorphisms have at some time been actively maintained by selection.

Actually, both the situations suggested by Robertson are typical of the heterochromatic pattern of chromosomes in wild populations of the perennial plant *Trillium kamtschaticum*. Extensive cytological studies of this plant by Haga and his associates^{16,17} have shown that several chromosome types are segregating within a large population, while different types are fixed in small isolated populations. Indeed, Robertson's suggestion is pertinent if isolation between sub-populations is nearly complete and if the mutation rate for neutral isoallelic variations is sufficiently high that more than one new mutant appears within a large population each generation. The chromosome polymorphism in *Trillium* can be explained by assuming a relatively high mutation rate per chromosome and very low migration rate per generation for this plant.

Mutation and Mobility

On the other hand, it is possible that in animal species such as *Drosophila*, mouse and man well able to migrate, no local population is sufficiently isolated to prevent the entire species or subspecies from forming effectively one panmictic population. In his study on "isolation by distance", Wright¹⁸, using his model of continuum over an area, has concluded that the total species differ little from a single panmictic population if the size of the "neighbourhood" from which parents come is more than 200.

Recently, Maruyama^{19,20} made an extensive mathematical analysis of the stepping stone model of finite size. He worked out the exact relationship between local differentiation of gene frequencies and the amount of migration. His results show that in the two dimensional stepping stone model, if N is the effective size of each colony and m is the rate at which each colony exchanges individuals with four surrounding colonies per generation, then marked local differentiation is possible only when Nm is smaller than unity (assuming a large number of colonies arranged on a torus). This is a very severe restriction for migration between colonies because the number of individuals which each colony exchanges with surrounding colonies must be less than an average of one per generation, irrespective of the size of each colony. For the model of continuous distribution of individuals over an area, this condition is equivalent to $N_\sigma < \pi$, where N_σ is the average number of individuals within a circle of radius σ , the standard deviation of the distance of individual migration in one direction per generation. If, on the other hand, there is more migration, the whole population tends to become effectively panmictic. The transition from marked local differentiation to practical panmixis is very rapid for the distribution over an area, and it can be shown that if $N_\sigma > 12$, the whole populations behave as if it were a single panmictic population.

This means that when two or more alleles happen to be segregating within a species, their frequencies among different localities far apart from each other are nearly the same. For animals with separate sexes, it is expected that at least several individual males and females usually exist within a circle of radius σ and the condition $N_\sigma > 12$ is therefore almost always met by widely distributed and actively moving animals. Maruyama also showed that when isolation is more complete and different alleles tend to fix in different local populations, they are connected by zones of intermediate frequencies. The overall pattern then mimics a gene frequency cline resulting from selection, even if alleles are in fact neutral.

Heterozygosity and Probability of Polymorphism

Let us now consider the number of neutral isoalleles maintained in a finite population. Kimura and Crow²¹ have shown that if u is the mutation rate per locus (cistron) for neutral mutants, the effective number of alleles maintained in a population of effective size N_e at equilibrium is

$$n_e = 4N_e u + 1 \quad (3)$$

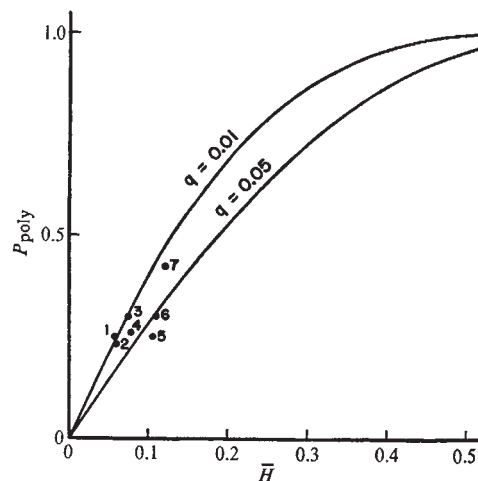


Fig. 1 Relationship between the probability of polymorphism (P_{poly}) and the average heterozygosity $\bar{H}$. The two curves represent the theoretical relationship based on the neutral polymorphism theory; the dots represent observed values. 1, *Limulus polyphemus*; 2, *Peromyscus polionotus*; 3, *Homo sapiens*; 4, *Mus musculus* (Denmark); 5, *Drosophila persimilis*; 6, *Mus musculus* (California); 7, *Drosophila pseudoobscura*.

In deriving this formula, it was assumed that the possible number of allelic states per locus is so large that whenever a mutant appears it represents a new, not pre-existing allele. The effective number of alleles given by formula (3) is equal to the reciprocal of the average homozygosity and is the number estimated by the ordinary procedure of allelism test. Then the average heterozygosity is

$$\bar{H} = 1 - 1/n_e = 4N_e u / (4N_e u + 1) \quad (4)$$

This is the mean frequency of the heterozygotes averaged over all cases including monomorphic and polymorphic cases.

We shall call a population "monomorphic" if the sum of the frequencies of "variant" alleles is q or less. Then it can be shown²² that the probability of a population being monomorphic is

$$P_{mono} = qn_e^{-1} \quad (5)$$

when n_e is the effective number of alleles. This formula has been derived under the same condition as formulae (3) and (4). Note that the value of q is arbitrary but a reasonable value is 0.01. If, however, a sample from each locality consists of only a dozen or so individuals, $q=0.05$ may be more appropriate. The probability that a population is polymorphic ($1 - P_{mono}$) is then

$$P_{poly} = 1 - q\bar{H} / (1 - \bar{H}) \quad (6)$$

Fig. 1 illustrates the relationship between the probability of polymorphism and the average heterozygosity at two levels of q (0.01, 0.05) together with some observed values compiled by Selander *et al.*²³ in their Table 3. The agreement between theoretical and observed values is satisfactory.

From this discussion it can be seen that for actively moving animals such as *Drosophila*, mouse and man, the frequency and the observed pattern of polymorphism can be explained by assuming effective migration such that

$$Nm > 4 \quad (7)$$

between adjacent local populations and also assuming a low mutation rate per cistron for neutral isoalleles such that

$$4N_e u \approx 0.1 \quad (8)$$

In formulae (7) and (8), N refers to the effective size of the local population (colony) while N_e refers to the effective size of the total Mendelian population such as species or subspecies.

When these two conditions are met, the effective number of alleles is on the average 1.1 and at each of the polymorphic

loci (constituting roughly 0.3 of all loci) a particular allele takes a high frequency such as 0.8 while the remaining alleles exist in frequencies less than 0.2 in all. Furthermore, frequencies of those alleles among different local populations are about the same throughout the species.

Relative Neutral Mutation Rate

As mentioned already, we know from studies of molecular evolution the average rate of amino-acid substitution. Then, using equation (1), we can infer that the mutation rate for neutral isoalleles is $u_{aa} = 1.6 \times 10^{-9}$ per amino-acid site per year. If the average cistron responsible for isozyme polymorphisms consists of 300 amino-acids and if 0.3 of amino-acid changes lead to change of the electric charge

$$u = 1.6 \times 10^{-9} \times 300 \times (0.3) = 1.6 \times 10^{-7}$$

per year. If the fraction of neutral mutants is less among mutants that can be detected by electrophoresis than among those that cannot be so detected, this figure is an overestimate. The same applies if the changes that can be detected by electrophoresis are restricted to amino-acids that are exposed to the surface of the protein molecule. It is therefore possible that the true mutation rate is much lower than this and in the following treatment we take $u = 10^{-7}$.

For species such as the mouse, with possibly two generations per year, the mutation rate per generation for neutral isoalleles detectable by electrophoresis is half as large, while for man it should be some twenty times as large. Then the effective population number that satisfies formula (8) is $N_e \approx 0.5 \times 10^6$ for the mouse and $N_e \approx 1.3 \times 10^4$ for man. The effective number here refers to the species or subspecies in the course of evolution.

We note that if the mutation rate u is constant per year, then the product $N_e u$ should be less variable among different organisms than its components N_e and u , because the species with short generation time tends to have small body size and attain a large population number, while the species which takes many years for one generation tends to have a small population number. At any rate, $u = 10^{-7}$ per year is much lower than the standard figure of 10^{-5} per generation even for man and this suggests that, in general, neutral mutants constitute a small fraction of all the mutants at a cistron. Thus, we consider this as one important revision to earlier work¹ in which it was assumed that the neutral mutation rate per locus was high.

We must emphasize, however, that most mutants that spread into the species are neutral, even if the neutral mutants constitute a small fraction of the total mutants at the time of occurrence. Those mutants that are destined to spread to the species take a long time until fixation and on their way take the form of "protein polymorphism".

If most protein polymorphisms constitute a phase of molecular evolution, then the behaviour of molecular mutants in a population is crucial for an understanding of the polymorphism. It was shown by Kimura and Ohta²⁴ that for a selectively neutral mutant, it takes about $4N_e$ generations to reach fixation in the population (excluding the cases of eventual loss). We can also compute, using the solution of Kimura²⁵, the average number of generations that have elapsed since the appearance of a neutral allele which happens to have reached frequency 0.5. It turns out that this is about $(1.25)N_e$ generations.

One further factor we must consider is "associative overdominance". When truly overdominant loci are distributed over the genome, they will cause neutral loci to behave as if they were overdominant²⁶. As we have shown (unpublished), this will somewhat prolong the time spent by a neutral mutant at intermediate frequencies, but this has no effect on the rate of mutant substitution in evolution. The associative overdominance, however, will play an important role when a small number of chromosomes are extracted from natural

populations and rapidly multiplied for an experiment. In this case, spurious "balancing selection" will be observed.

Polymorphism in Living Fossils

Returning to the problems of evolutionary time, we note that the average number of generations between two consecutive fixations of mutants at a given locus (cistron) in the course of evolution is $1/u$. This is roughly ten times as long as the time taken for an individual mutant to reach fixation if $4N_e u = 0.1$. It may be interesting to ask, then, how long it takes until new mutants accumulate in the species causing detectable change in the fraction P_d of proteins. If we denote by T_d the average length of time for such change, then

$$T_d = -(1/u) \log_e(1 - P_d) \quad (9)$$

According to Selander *et al.*²⁷, two Danish subspecies of the house mouse differ at 32% of their loci. Putting $P_d = 0.32$ and assuming $u = 10^{-7}$ per year, we obtain $T_d \approx 3.9 \times 10^6$ yr. The time since divergence of these two subspecies from their common ancestor is given by $T_d/2$ or roughly 2 million years.

In our view, protein polymorphism and molecular evolution are not two separate phenomena, but merely two aspects of a single phenomenon caused by random frequency drift of neutral mutants in finite populations. If this view is correct, we should expect that not only genes in "living fossils" have undergone as many DNA base (and therefore amino-acid) substitutions as corresponding genes in more rapidly evolving species as predicted by Kimura⁵, but also they are equally polymorphic and heterozygous at the protein level. A study by Selander *et al.*²³ on the variation of the horseshoe crab at the protein level seems to support this view.

At the moment, our observations are limited to a few organisms, and we do not know how typical their heterozygosities are. It is possible that for organisms with short generation time and small effective population number, the level of heterozygosity is much lower (because of a very small $N_e u$).

The neutral mutation-random drift theory allows us to make a number of definite quantitative as well as qualitative predictions by which the theory can be tested. We hope that through this process we will be able to gain deeper understanding of the mechanism of evolution at the molecular level and will be emancipated from a naive pan-selectionism.

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Long Range Forces in Muscle

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Calculations suggest that electrostatic and dispersion forces play a part in determining the stability of the muscle filament lattice, but it is unlikely that this is a complete explanation.

SOME biological fibres consist of bundles of rod-shaped molecules of filaments assembled in parallel with the fibre axes and packed together in a nearly hexagonal manner. In the native state, the filaments within bundles are frequently at an equilibrium separation from each other, and this can readily be varied by changes in the pH and ionic strength of the bathing medium.

What are the forces responsible for determining the particular equilibrium separation adopted? The classic work of Bernal and Fankuchen¹ on artificial gels of certain rod-shaped viruses entailed the use of low angle X-ray diffraction to measure the interfilament separation and its variation with pH and ionic strength. The results suggested that the intermolecular forces are those usually invoked to account for the behaviour of colloids and this was lent support by the calculations of Levine (reported in ref. 1) which were later, however, shown to contain a conceptual flaw. The essential behaviour of this system can be summed up quite simply. At a fixed pH, an increase in the ionic strength of the interfilament medium is accompanied by a decrease in the separation between filaments until a minimum separation is achieved at high ionic strength (Fig. 1). Second, if the pH is decreased from about 7, the interfilament separation also decreases until a minimum separation occurs at about pH 3.5. Further decrease in the pH leads to an increase in the separation (Fig. 5).

A somewhat similar behaviour has now been recognized in muscle as a result of the work of Rome^{2,3}, who also used

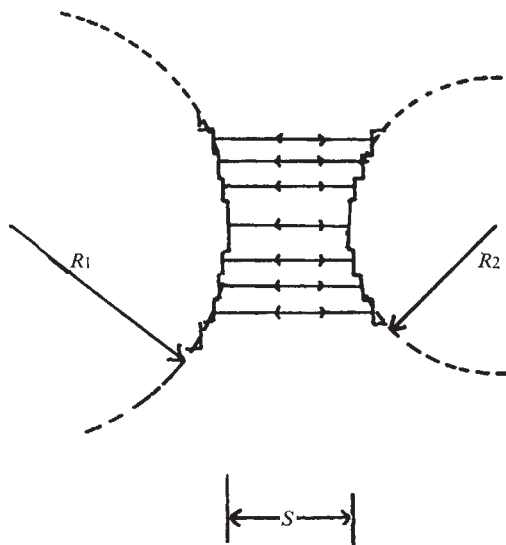


Fig. 1 Schematic representation of the calculation of the electrostatic repulsion between two cylinders of radii R_1 , R_2 by dividing the surface into strips parallel to the cylinder axis.

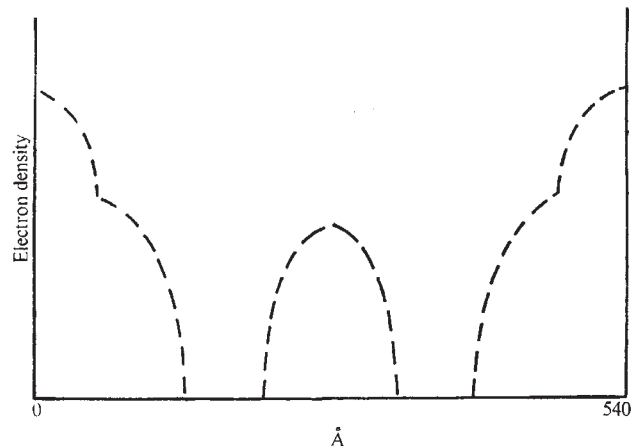


Fig. 2 Profile of projected electron density from the centre of one thick filament through a thin filament to the centre of the nearest neighbour thick filament. The electron density is projected first down the filament axis and then on to one side of the hexagonal unit cell. Note the profile of the thick filament (from Miller and Tregear¹⁶).

low angle X-ray diffraction to measure interfilament separations. In muscle the dependence of the separation on pH is qualitatively the same as that in the rod-shaped viruses but the form of the effects of ionic strength is more complicated and depends on whether the muscle is living, glycerinated or skinned²⁻⁴. Supporting but still inconclusive evidence that electrostatic interactions are important comes from the observation⁵ that pH and ionic strength alter the mechanical response of muscle and that the contractile properties are sensitive to different anions in the bathing medium⁶. Elliot^{7,8} has developed the idea that the equilibrium arises from the balance of electrostatic and dispersion forces and has concluded from numerical calculation of these quantities that in muscle a sufficiently stable minimum is predicted at a reasonable enough interfilament separation to justify the idea that these forces are relevant.

It has also been suggested⁸ that changes in the electrostatic interaction between filaments together with a mechanism which preserves a constant volume of the muscle sarcomere are responsible for the longitudinal force in all types of muscle contraction. Clearly it is important to investigate the molecular interactions involved, and we now show that expressions for the electrostatic and dispersion forces between filaments can be expressed in closed form with a negligible loss in accuracy. We have also confirmed that the muscle filament lattice may well be stabilized by the balance between electrostatic and dispersion forces, but there is evidence that this is not the whole story.

Interfilament Forces

There is no satisfactory calculation of the electrostatic repulsion between two parallel charged cylinders (radii R_1 , R_2) in an ionic medium at arbitrary ionic strength. When the Debye-Huckel screening length, $\lambda \ll R_1, R_2$, the problem is, however, tractable. The excluding envelope of myosin filaments is not less than 100 Å in radius and in physiological

conditions $\lambda \approx 10 \text{ \AA}$, corresponding to a molarity of salt solution of about 0.1, so that the above condition is fulfilled. The essence of the approximation is that the lines of force between the surfaces are not appreciably divergent. The cylinders may then be thought of as divided into flat strips parallel to the axes of the cylinders and the total force between them is the sum of the interactions between corresponding strips on the two cylinders (Fig. 1). Thus the problem is reduced to calculating the repulsion between two flat strips (a very well known result⁹) and summing them.

For two strips of width Δy at a separation x , the repulsive force per unit length is⁹

$$P = \Delta \gamma c \gamma_1 \gamma_2 \lambda^{-2} \exp \left(\frac{x}{\lambda} \right) \quad (1)$$

where

$$\gamma_i = \tanh \left(\frac{e \Psi_{oi}}{4 K_B T} \right)$$

Ψ_{oi} = surface potential of cylinder i

$$c = \frac{8}{\pi} \left(\frac{K_B T}{e} \right)^2 \epsilon_s$$

ϵ_s = dielectric constant of water ~ 80

$$\lambda = \left(\frac{8 \pi e^2}{\epsilon_s K_B T} \right)^{-\frac{1}{2}} I_c^{-\frac{1}{2}}$$

I_c = ionic strength.

The summation can be replaced by an integration yielding the electrostatic repulsive force at a separation between surfaces s

$$F_{e,s} = c \gamma_1 \gamma_2 \left(\frac{2 \pi R_1 R_2}{\lambda^3 (R_1 + R_2)} \right)^{\frac{1}{2}} \exp(-s/\lambda) \quad (2)$$

an expression of much greater convenience than one obtained by performing the integration numerically and one which is only negligibly less accurate.

In its most general form the calculation of the attraction between the two cylinders due to dispersion forces is also extremely difficult, for the quantity required is the mean force potential in water rather than the dispersion force in empty space.

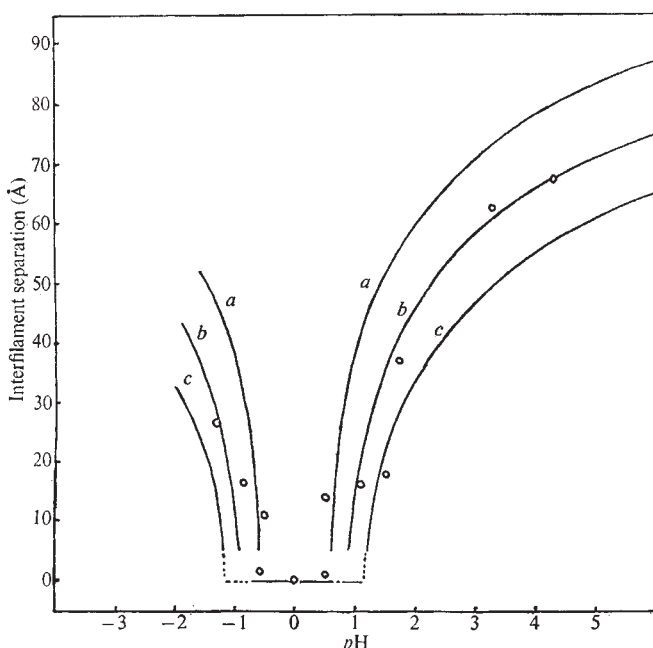


Fig. 3 Interfilament surface to surface separation plotted as a function of pH. $\circ$, Experimental points taken from Rome³; —, theoretical curves. a , $\delta = (\Gamma/K_B T)Z^{-2} = 20$; b , $\delta = (\Gamma/K_B T)Z^{-2} = 50$; c , $\delta = (\Gamma/K_B T)Z^{-2} = 100$.

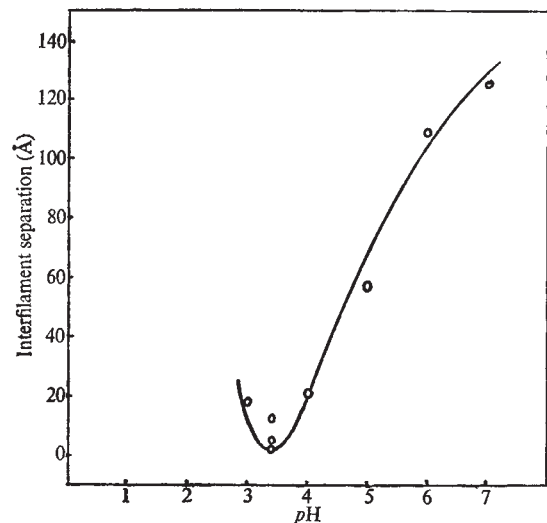


Fig. 4 Interfilament surface to surface separation for TMV plotted as a function of pH.

In principle, the empty space attraction is obtained by summation of the interaction of the individual atoms or groups comprising the two cylinders and in practice by integrating and assigning some mean interaction strength Γ to the aggregates⁷. The integration can be performed numerically⁷ but for small s the asymptotic functional form of the force can be quite rigorously written as

$$F_{d,vacuo} = - \left(\frac{\pi^2}{16} \right) \left(\frac{2 R_1 R_2}{R_1 + R_2} \right)^{\frac{1}{2}} \Gamma_s^{-5/2} \quad (3)$$

Furthermore, *in vacuo*, Γ can be shown quite easily to be

$$3/4 \bar{U} \bar{\alpha}_1 \bar{\alpha}_2 \quad (4)$$

where $\bar{U}$ is the average ionization potential of the constituent atoms, approximately 12 eV, and $\bar{\alpha}_1$ the mean polarizabilities of the aggregates.

The dielectric effect of the surrounding water on dispersion forces cannot be adequately assessed either experimentally or theoretically, although it is likely to be small because the frequency of the photons taking part in the interaction is much greater than that of a possible reorientation of dipoles. The effect of the finite high frequency polarizability of the water can, however, be considered and shown to yield an effective interaction of the same functional form as *in vacuo* but reduced in strength by a factor¹⁰

$$\left(1 - \frac{\bar{\alpha}_w}{\bar{\alpha}_1} \right) \left(1 - \frac{\bar{\alpha}_w}{\bar{\alpha}_2} \right) \quad (5)$$

where $\bar{\alpha}_w$ is the average polarizability of bulk water. Thus effectively

$$\Gamma = 3/4 \bar{U} (\bar{\alpha}_1 - \bar{\alpha}_w) (\bar{\alpha}_2 - \bar{\alpha}_w) \quad (6)$$

We are now in a position to discuss the behaviour of systems of muscle fibres. Although equation (3) is strictly correct in the limit of small s , it gives a very good representation of the attractive force for all $s < R_1, R_2$ and this leads to the first interesting and important result. Equations (2) and (3) can be combined to give one, the solution of which is the equilibrium separation of cylinders s_0 and it is obvious, provided s_0 is small, that the equilibrium separation is independent of the size of the cylinders because R_1 and R_2 appear in the same functional form on both sides of the equation.

We shall now consider the case of the interaction of similar cylinders radius R , corresponding to the virus gel or the muscle filament lattice in its stretched state (no overlap of

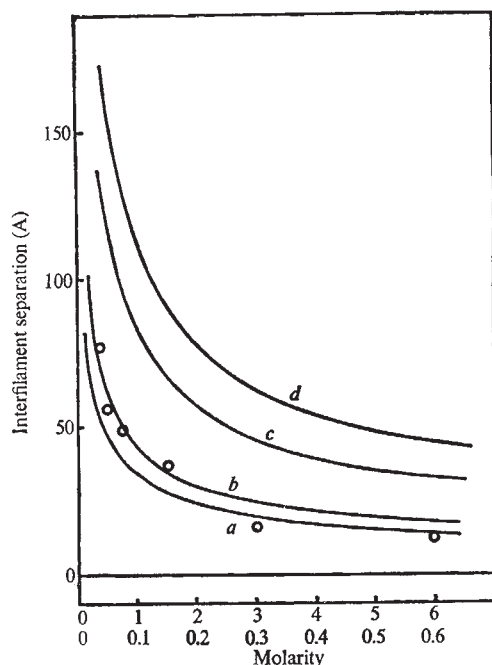


Fig. 5 Interfilament surface to surface separation plotted as a function of molarity. $\circ$, Bernal and Fankuchen's experimental points molarity from 0–6. *a*, Theoretical separation plotted as a function of asymmetric electrolyte (corresponding to $(\text{NH}_4)_2\text{SO}_4$ used by Bernal and Fankuchen), molarity 0–6. $\Gamma/K_B T \gamma^2 = 25$. *b*, As *a* but $\Gamma/K_B T \gamma^2 = 2.5$. *c*, Theoretical separation of two cylinders plotted as a function of symmetric electrolyte, molarity 0–0.6. $\Gamma/K_B T \gamma^2 = 100$. *d*, As *c* but $\Gamma/K_B T \gamma^2 = 10$.

actin and myosin). Table 1 shows the attractive force between two cylinders calculated numerically compared with that predicted by equation (3). For separations less than R the agreement is good and we feel justified in using it instead of the numerical results. The crucial equation derived from (2) and (3), after putting in suitable values for the various constants, is

$$\left(\frac{\Gamma}{K_B T}\right) \lambda^{-1} \gamma^{-2} = 1.096 \xi^{5/2} e^{-\xi} \quad (7)$$

where λ is in Å and $\xi = s/\lambda$. Equation (7) is in a particularly convenient form because the curve represented by the right side is common to all values of the variables Γ , λ and γ , which greatly facilitates solution.

Numerical Values

Denbigh¹¹ has shown that the polarizabilities of many large molecules can be estimated by summing their bond polarizabilities and has given the best values for the different kinds of covalent bond met with in organic systems. His method, together with the established fact that the specific volumes of proteins are all approximately 0.73, allows a reasonably confident assessment of $\Gamma = 3/4 \bar{U}(\bar{\alpha} - \bar{\alpha}_w)^2$. It is not difficult to show

that for a number of proteins whose volumes are known fairly accurately the mean polarizability per unit volume is ~ 0.077 corresponding to a dielectric constant of ~ 2.4 . The mean polarizability of water can be estimated from the Clausius–Mosotti relation and is 0.049. It follows that at physiological temperatures, $\Gamma/K_B T \approx 0.28$. Under conditions appropriate to Rome's work, the molarity of the surrounding solution is 0.1 corresponding to $\lambda = 10$ Å.

Only γ and, by implication, ψ_0 remain to be assessed. For high surface potentials, $e\psi_0/4K_B T > 1$ and $\psi_0 > 100$ mV, γ adopts its asymptotic form, unity. This is another useful result because it suggests that the separation is a constant independent of ψ_0 and therefore of the pH. The relationship for smaller surface potentials between the total net charge Q on the myosin filament and pH is required as it is the former which indirectly appears in the theory but the latter which is the experimental quantity. Such a relationship between changes in pH and changes in net charge ΔQ can be estimated by appealing first to Kirkwood's equation¹²

$$\Delta Q = \sum_i \frac{N_i}{\left(1 + \frac{K_i^0}{H_1^+}\right)} - \sum_i \frac{N_i}{\left(1 + \frac{K_i^0}{H_2^+}\right)} \quad (8)$$

where N_i is the number of amino-acids of each kind present; K_i^0 are their respective dissociation constants; H_1^+ , H_2^+ are the hydrogen ion concentration at the differing pH values, and second to Hill's equation¹³ valid for small potentials

$$\Delta \psi_0 \approx \frac{2\Delta Q}{\epsilon_s L} \left(\frac{\lambda}{R}\right) \frac{K_0(R/\lambda)}{K_1(R/\lambda)} \text{ for } R/\lambda \text{ large} \quad (9)$$

where L is the length of the myosin filament and the K_0, K_1 are modified Bessel functions. The amino-acid composition of myosin¹⁴ suggests that for pH changes up to 3 above the isoelectric point, ΔQ is 12 mol of electronic charge per unit pH change per 10^5 g of protein. This gives an estimate for $\Delta \psi_0$ of about 10 mV per unit pH change. The surface potential is probably lower than this because not all groups are likely to be affected by the changing pH, and also because of the so-called "discrete charge effect"¹⁵. The latter refers to the binding of ions from solution which always causes a lowering of $|\psi_0|$. The size of these effects is a little difficult to assess but if the electrolyte is uni-univalent and the concentration of ions not too high the potential will certainly not change its order of magnitude.

One further complication must also be included in the theory. It seems likely that in any system of the kind under discussion there will be a layer of bound water thickness d , say, on the protein surfaces which excludes electrolyte and inhibits screening. This may be roughly accounted for by replacing the left side of equation (7) by

$$\frac{\Gamma}{K_B T} \lambda^{-1} \gamma^{-2} \exp(-2d/\lambda) \quad (10)$$

Theory and Observation

The minimum separation achieved at high ionic strength in Bernal and Fankuchen's study coincides approximately with the minimum of the curve of separation against pH, and such behaviour is predicted by the theory as summed up in equation (7). Figs. 4 and 5 show their results expressed as displacement for the minimum compared with our theoretical results. It is difficult to compare the two quantitatively because the ionic strength at which the variation of pH takes place is not stated; the qualitative agreement, however, is good. If changes in separation from some experimental or theoretical minimum are considered the behaviour is insensitive to the thickness of the bound water layer, d , and fairly insensitive to the parameter $\Gamma/K_B T$. They seem justified in deducing from their

Table 1 Attractive Force between Two Cylinders calculated Numerically compared with the Prediction of Equation (3)

Separation $\zeta = s/R$	$R^2 F_D(\zeta)$ (numerical)	$R^2 F_D(\zeta) = \frac{\pi^2}{16} \zeta^{-5/2}$ (equation 3)
0.133	86.06	95.06
0.267	13.73	16.76
0.400	4.56	6.13
0.500	2.40	3.49
0.533	2.01	2.97
0.6	1.41	2.21
0.667	1.04	1.67
0.7	0.86	1.51
0.800	0.596	1.07
0.9	0.40	0.803
0.933	0.368	0.754

results that there is some more or less well defined boundary of the protein and bound water comprising the virus.

The boundary envelope of a myosin filament is likely to be less well defined; evidence^{16,17} suggests that there is a compact core formed from the LMM and a rather more diffuse region comprising the HMM (Fig. 2). If the theory as developed here is at all relevant, it is difficult to escape the conclusion that the minimum in the separation curve (Fig. 3), achieved when $\psi_0=0$, gives the effective boundary of the filaments. The only parametric fitting that has been used in comparing Rome's results with the theory is to make the experimental and theoretical minima coincide. Provided that $\psi_0 < 50$ mV, $\tanh(e\psi_0/4K_B T)$ can be replaced by $Z=e\psi_0/4K_B T$ and $\delta=Z^{-2}/K_B T$. A number of theoretical curves are plotted for different values of δ . Ten mV per pH unit are equivalent, taking a value $\Gamma/K_B T=0.28$, to $\delta=28$. It is apparent that Rome's results fall nicely within the area indicated by the theory.

The minimum centre to centre distance observed by Rome is approximately 270 Å. This suggests that the excluding envelope of myosin is somewhat greater than the myosin diameter indicated by X-ray studies¹⁷ which is about 220 Å. The X-ray results give information about those parts of the cross bridges which are regularly arranged and it could be that the larger excluding envelope reveals that the outer ends of the cross bridges are disordered. It seems probable that the cross bridges are involved in determining the minimum inter-filament distance because of the observation that the closest approach in glycerinated muscle (when the cross bridges are known to reach right across to the thin filaments) is larger than that achieved in living muscle^{2,3}. Another possible source of complication in muscle is the M-line and I-line which may restrict the free lateral movement of the thick and thin filaments respectively. The effect of these could be investigated

by detailed observations on the shape of the minimum of the curve relating pH to interfilament distance.

Our calculations do not account for all the varieties of behaviour of the interfilament distance at different ionic strengths²⁻⁴. Many of the curves reported by Rome have the same form as our theoretical curves (for example, Fig. 4) apart from at those values of ionic strength where the myosin filaments are known to be disaggregating¹⁸. Other curves, however, do not give such a good fit and, together with the indications from the variation with pH, suggest that though the balance between electrostatic and dispersion forces is undoubtedly important in determining the stability of the muscle filament lattice, other factors will probably have to be taken into account.

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Neovitamin B₁₂ Identified

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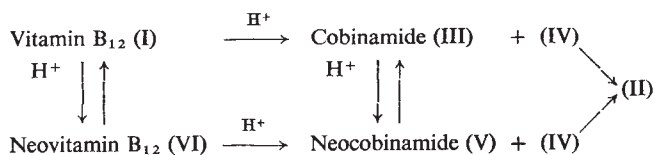
Neovitamin B₁₂, formed from and converted into vitamin B₁₂ in conditions of high acidity, has been shown to be cyano-13-epicobalamin.

WHEN vitamin B₁₂ (I) is treated with anhydrous trifluoroacetic acid at room temperature for several hours, cleavage of the phosphate-propanolamine bond occurs to give the 2' and 3'-nucleotides (II) and a mixture of cobinamide (III) with a closely related compound which we have termed neocobinamide¹. (The latter substance is identical with the Factor Ia reported several years ago by Friedrich and Bernhauer².) Cobinamide and neocobinamide have been isolated, and when treated with trifluoroacetic acid (and various other highly acidic reagents) each gave an equilibrium mixture of the two compounds. Similar equilibria have been observed with corrinoid monocarboxylic acids, including cobyric acid, and with a dicarboxylic acid³⁻⁵.

When the cleavage reaction with trifluoroacetic acid was interrupted after 2 h, a third red component, with a lower R_F than that of vitamin B₁₂, was observed. The progress of the reaction is shown in Fig. 2, which illustrates the intermediate formation of this new compound, and the eventual destruction of both this and vitamin B₁₂ to give, after about 24 h, a mixture of cobinamide and neocobinamide practically uncontaminated by other corrinoids. The new compound formed in maximal amount after 2 h was isolated by preparative paper chromatography and was crystallized as red prisms (5%) from acetone-water containing a trace of hydrogen cyanide.

The new compound, which was termed neovitamin B₁₂ (cyanoneocobalamin, VI), was shown to be very closely related to vitamin B₁₂. The infrared spectrum was almost identical to that of vitamin B₁₂ and, in particular, showed the weak but sharp absorption band (2,135 cm⁻¹) assigned to the cyanide ligand. Hydrolysis of neovitamin B₁₂ with cerous hydroxide in the presence of cyanide ions⁶ gave neocobinamide. Treatment of neovitamin B₁₂ with trifluoroacetic acid for a short time gave a mixture of the 2' and 3'-nucleotides (II) together with neocobinamide (principally), cobinamide, and

a small amount of vitamin B₁₂. The latter was identified spectroscopically, electrophoretically, chromatographically, and by microbiological assay. Thus the equilibration observed with simpler systems (for example, cobyric acid $\rightleftharpoons$ neocobyric acid) also occurs with the vitamin itself, although phosphate ester cleavage is a competing reaction in this case. Direct evidence for the intermediacy of the cyclic phosphate (IV) in the reaction with trifluoroacetic acid has recently been obtained, and Fig. 1 can now be extended as follows:



That vitamin B₁₂ and neovitamin B₁₂ were isomers, and the parent compounds of two isomeric series, was suggested by the equilibration experiments. Further evidence for this view came from the mass spectra of heptamethyl cobyrate and heptamethyl neocobyrate. Although the molecular ions were not observed, heavy fragment ions (including m/e 1004 and m/e 1006) occurred at the same positions in both spectra.

The electronic spectra of vitamin B₁₂ and neovitamin B₁₂ were quite similar to one another (Fig. 3), although close inspection revealed significant differences, which were observed between the neo and normal series in general. On paper chromatograms, compounds of the neo-series were always of a darker red colour than those of the normal series. The chiroptical properties of the two compounds (and the two series) were, however, quite distinct as illustrated by circular di-

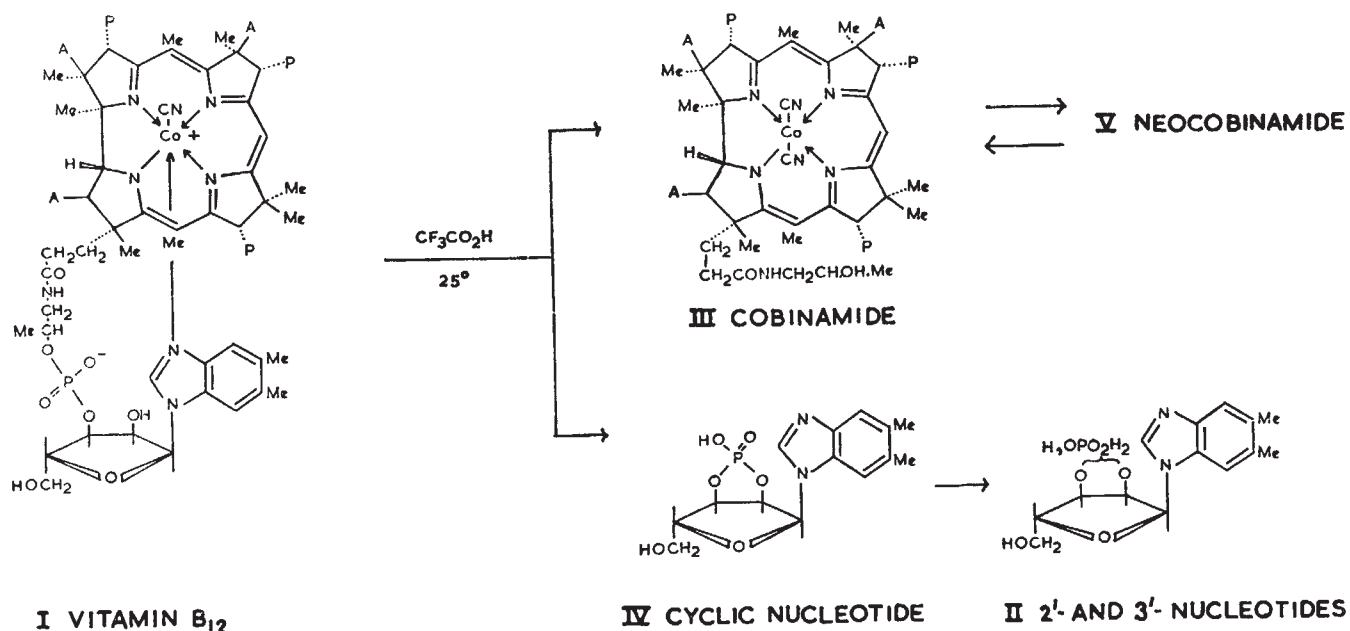


Fig. 1 Cleavage of vitamin B₁₂ in highly acidic media. (A = -CH₂CONH₂, P = -CH₂CH₂CONH₂).

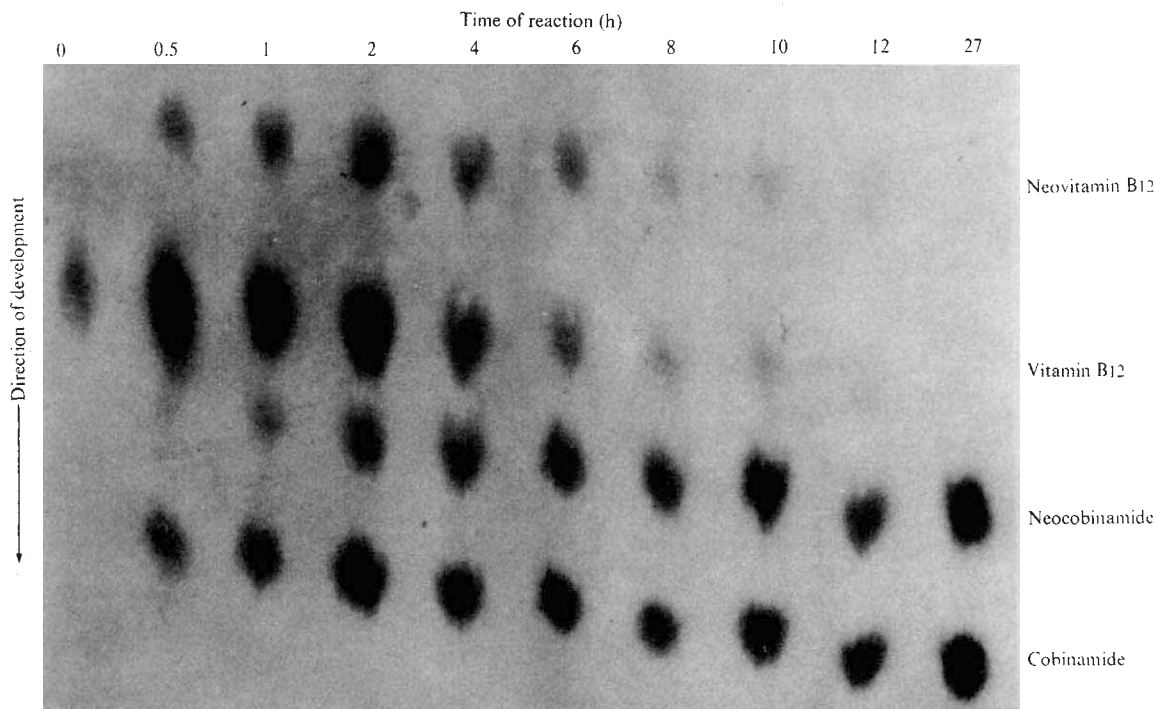


Fig. 2 Progress of the reaction between vitamin B₁₂ and trifluoroacetic acid at 25°C. (Whatman 3 MM paper, downward development for 48 h with butan-2-ol saturated with water, and containing a trace of hydrogen cyanide.)

chromism spectra (Fig. 4). It was concluded⁵ that the inter-conversion: normal series $\rightleftharpoons$ neo-series involved a configurational, or, possibly, a conformational change such as a change in the skew of one of the $\beta\beta'$ -bonds. An X-ray crystallographic analysis was undertaken to settle the problem.

Crystallographic Investigation

Deep red crystals were grown from aqueous acetone containing a trace of hydrogen cyanide. They were mounted in the presence of, but not in contact with, some of their mother liquor, in thin-walled glass capillaries, and preliminary X-ray data were recorded as follows: crystals of neovitamin B₁₂, formula C₆₃H₈₈O₁₄N₁₄PCo + probably 18 H₂O; crystal class orthorhombic, space group P2₁ 2₁ 2₁; $a=24.98$, $b=22.06$ and $c=15.74$; $\rho_{\text{obs}}=1.338$ g/cm³, $\rho_{\text{calc}}=1.343$ g/cm³. The intensities of the reflexions from planes with a spacing greater than 1 Å were collected with CuK α radiation on a Hilger and Watts automatic four circle diffractometer and yielded a final set of 2,989 independent reflexions whose background corrected intensities were larger than four times their standard

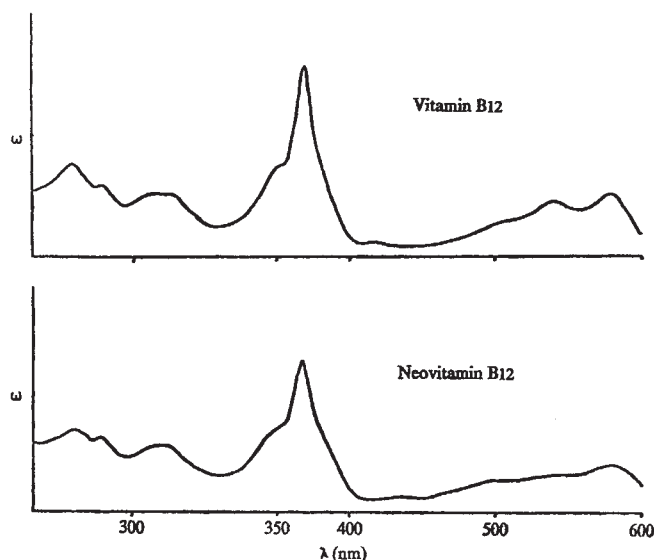


Fig. 3 Electronic spectra of vitamin B₁₂ and neovitamin B₁₂ in 0.1 M potassium cyanide.

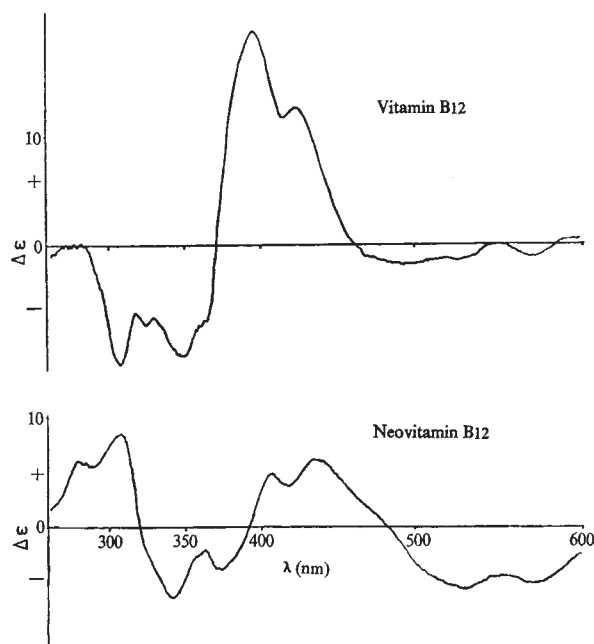


Fig. 4 Circular dichroism of vitamin B₁₂ and neovitamin B₁₂ in 0.1 M potassium cyanide.

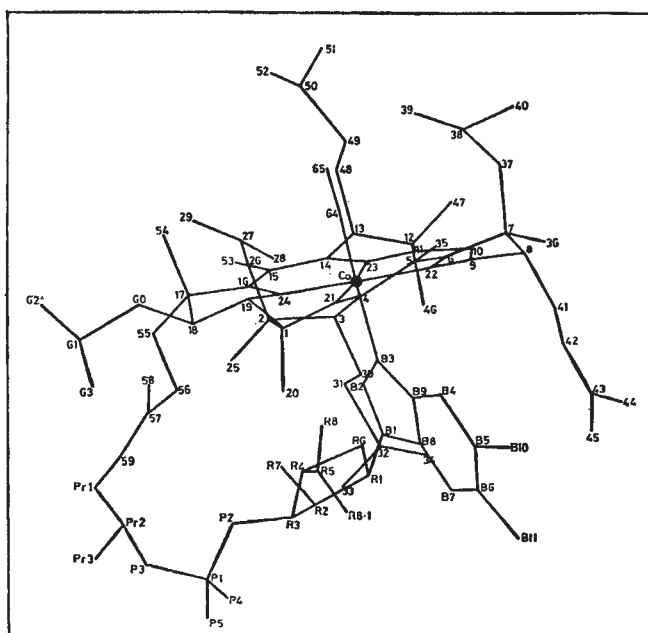


Fig. 5 The atomic positions observed in neovitamin B₁₂, viewed down the crystallographic c axis.

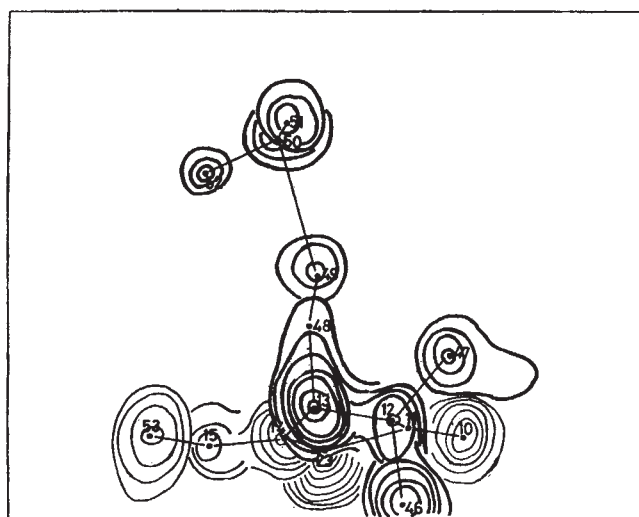


Fig. 6 Composite view of the electron density over the C-13 propionamide group taken from the first map calculated with "anomalous" phases.

deviation. One pair of Bijvoet related reflexions was measured for each of these. The data were Lorentz-polarization and absorption corrected and then put on an absolute scale using Wilson's method.

The anomalous dispersion effects observed were significant, so they were used to improve the phase angles evaluated from the cobalt atom position alone using a recently developed programme. This applied the method suggested by Ramachandran and Raman⁷ and took the solution nearest to the cobalt phase as being the correct one. From a three-dimensional sharpened Patterson synthesis, two enantiomorphous arrangements of cobalt positions were derived, both of which were used to evaluate "anomalous" phases. Of the two resulting maps of the electron density, one had higher and better resolved electron density peaks than the other and was clearly the correct solution. From this first map it was possible to place eighty-two of the ninety-three atoms of the molecule in the correct absolute configuration, the positions of the others being determined in the course of the refinement of the structure. R is now 16.9%. The molecule is shown projected down

LETTERS TO NATURE

PHYSICAL SCIENCES

Quasars or Blue Stars?

THE identification of southern hemisphere radio sources is in progress at the Yale University Observatory. The first epoch plates of the Yale-Columbia programme for determining the proper motion of southern stars have been examined for

identifications, and I have already reported forty-one suggested identifications¹, discussing the method and techniques in detail. In this communication I wish to point out that two radio sources, PKS 0320-37 and 1302-49, usually identified with the galaxies NGC 1316 and NGC 4945, may instead be quasi-stellar sources in the direction of those galaxies.

Two 17 × 17 inch plates were exposed simultaneously through objective gratings on each of blue and yellow, which cover

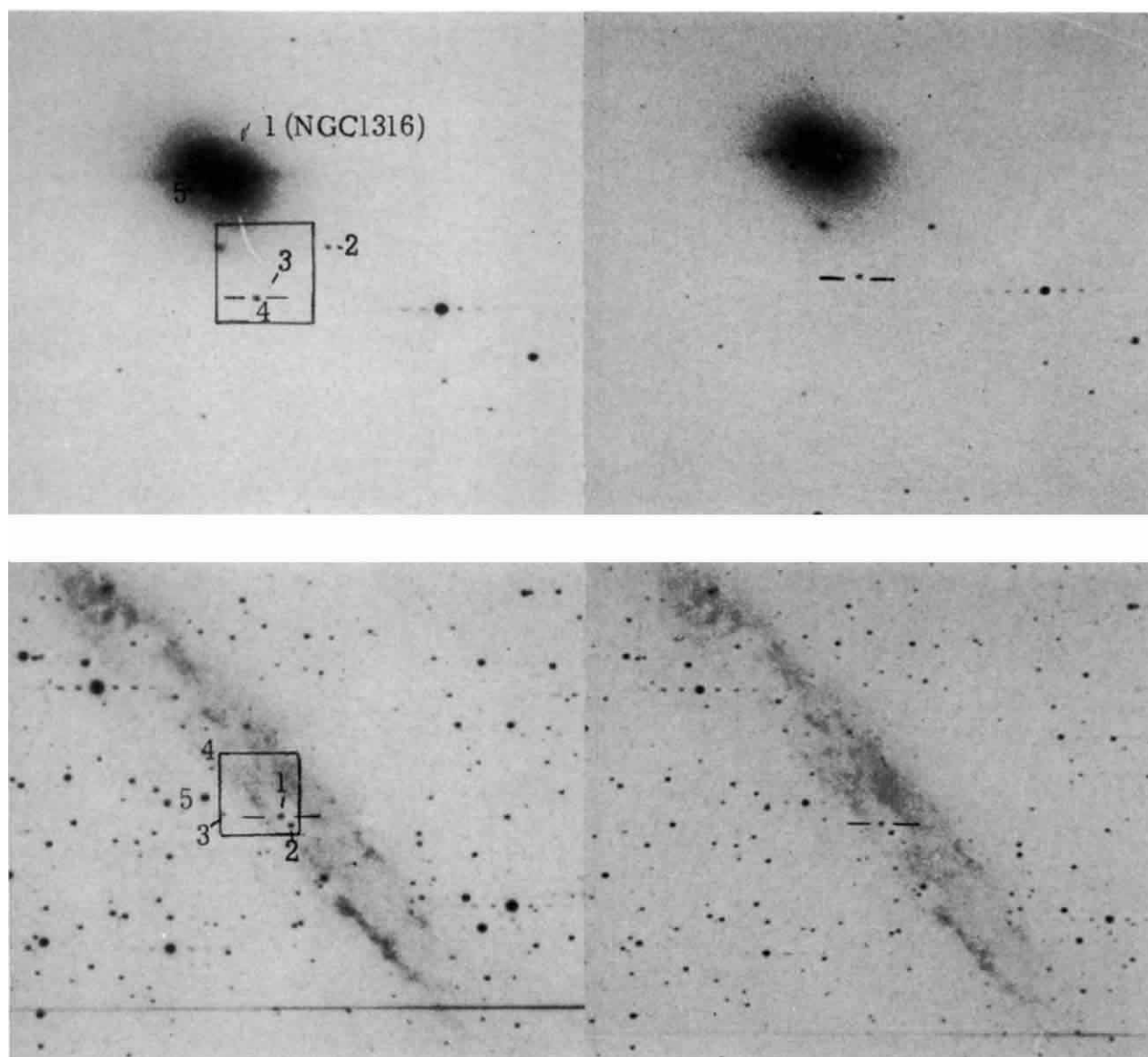


Fig. 1 Top, PKS 0320-37; bottom, PKS 1302-49. Blue plate prints on left and yellow plate prints on right. The chart is about 10' × 10'; north is to the top and east is to the left.

6.1 × 6.1 degrees. Plates were exposed for 2 h (with typical limiting magnitude of 19.5 for the blue and 18.5 for the yellow plate) followed by a 2 min secondary exposure. A computer plotted the positions of about thirty reference stars and of the radio source, surrounded by a box representing the possible error in its position. A direct overlay of the plate on the plot, achieved by aligning the reference stars, gave the field of the radio source.

A comparison of the position of PKS 0320-37 and 1302-49, given by Bolton *et al.*², with the NGC catalogue suggested that the identifications were NGC 1316 and NGC 4945 respectively. Shimmins *et al.*³ provided a more accurate radio position for PKS 1302-49 and gave an estimated maximum error of 0'.4.

I have found, however, that the error boxes of PKS 0320-37 and 1302-49 each contain a stellar blue object. Reproductions from both the blue and yellow plate prints are given in Fig. 1. It is well known that the radio centroid of a radio emitting galaxy does not usually coincide with the optical galaxy and often there are two radio centroids on opposite sides of the optical system. But in neither of these two cases is there a correspondent opposite source.

Bolton *et al.*² have said that PKS 1302-49 has a strong positive curvature spectrum which has been explained in terms of synchrotron decay in evolving sources; they believe that the radiation is confined to a small region near the nucleus of the galaxy. An examination of the optical photographs reveals no obvious nucleus of NGC 4945 at blue and yellow sensitive wavelengths. It is classed as a core-halo object by Ekers⁴, and the halo has a steeper spectrum than the core; a radio diameter for one component is 0'.5.

NGC 1316 may be associated with PKS 0320-37, but it falls outside the 0'.6 × 0'.6 square of uncertainty given in ref. 2; it is also a double radio source. Two optical objects, both in the error box, are a 17m.5 stellar object with strong blue excess and an 18m.0 star with no obvious blueness. The faint image of a galaxy in the box is the secondary exposure of NGC 1316. Positions of all optical objects measured are indicated on the finding charts listed in Table 1.

Table 1 Optical Position

Name	α	(1950)	δ	Remarks
PKS 0320-37				
Radio position	03 h 20 m	42.0 s	-37° 25' 00"	
Optical position 1		46.92	23 06.7	NGC 1316
2		37.21	24 30.2	
3		42.73	25 11.4	
4		43.94	25 23.4	Blue stellar object 17.5 m
5		49.91	23 21.8	
PKS 1302-49				
Radio position	13 h 02 m	33.1 s	-49° 12' 00"	
Optical position 1		31.35	12 21.2	Blue stellar object 17.3 m
2		30.21	30.9	
3		37.81	19.5	
4		39.36	11 30.3	
5		39.92	12 01.9	

If either PKS 0320-37 or 1302-49 is confirmed as a QSO, the significance of such a QSO so close to the bright galaxies is greatly enhanced. The determination of its redshift would probably confirm whether or not one of these two objects is related to NGC 1316 or NGC 4945. Absorption lines from extended gas clouds of the galaxies and lines intrinsic to the two radio sources would give the same or different redshift, and it might then be possible to set a lower limit on the distance to the QSO.

The probability of finding a QSO in near alignment with a galaxy is very small, but in these two cases it cannot be ruled

out without further investigation. It would be very useful to obtain spectra of these two objects and resolve their identity.

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Earth's Dust Cloud

ABOUT 10 yr ago, it was suggested on the basis of theoretical¹ and observational² evidence that there is a high concentration of interplanetary dust near the Earth (generally called the geocentric dust cloud). Although more recent theoretical studies³⁻⁷ and zodiacal light observations^{7,8} indicate that the existence of such a cloud is unlikely, it has been revived⁹ to explain the observed Doppler shifts in the zodiacal light¹⁰.

During a 4 yr study of the Gegenschein¹¹⁻¹³ I have shown that concentrations of interplanetary dust near the Earth would be detectable by means of the Earth's shadow. In this communication I describe the application of such a test to the hypothetical geocentric dust cloud. The first step in building a model is to determine the radial density distribution and phase function of the dust particles in the cloud. Beard and Whipple suggested that the concentration falls off roughly as the inverse 1.5 power of the geocentric distance, out to a distance of about 100 Earth radii (R_E). Vanysek and Harwit base their calculations on a physically less realistic model with constant density also out to about 100 R_E . The phase function found^{12,13} for the particles producing the Gegenschein is 0.023 mag/deg. This will be used, although the phase function assumed here is not critical.

The predicted brightness including the effect of the Earth's shadow for each of the suggested radial distributions is shown in Fig. 1. Calculations have been made for the case where the antisolar point is directly overhead (zenith distance equals 0°) and also at zenith distance 35°. The asymmetries in the shadow shapes for 35° zenith distance are due to the fact that

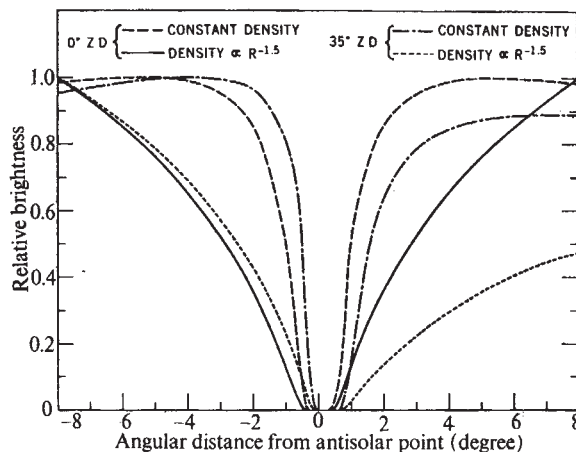


Fig. 1 Calculated relative brightness for the geocentric dust cloud assuming a phase function of 0.023 mag/deg for two different radial mass distributions and zenith distances. In all figures the negative direction is toward the sunlit side of the Earth.

the contributions due to the umbra and penumbra vary markedly¹²⁻¹⁴.

It is immediately apparent that the geocentric dust cloud cannot make an important contribution to the Gegenschein brightness, since no light can be reflected from inside the Earth's umbra (light scattered into the umbra by the atmosphere is negligible^{13,14}). When the antisolar point is at the zenith the black hole due to the umbra is circular and 0.6° in diameter.

Fig. 2 shows two observations of the Gegenschein corrected for atmospheric extinction and airglow made with a pulse counting photoelectric photometer at the McDonald Observa-

tory on February 7, 1970, at 2.5 h east and 2.2 h west of the meridian, with corresponding zenith distances of 37° and 33° respectively. The field of view for each observational point was 96 arc sec in diameter. The lack of any observed shadow immediately indicates that the contribution of the GDC must be small.

To set an upper limit on the contribution of the geocentric dust cloud to the Gegenschein it is necessary to build a two component model containing the contributions of both the geocentric dust cloud and the material outside the Earth's shadow. Models for 0° zenith distance are unsatisfactory since material within $6.9 R_E$ from the Earth's centre is in the

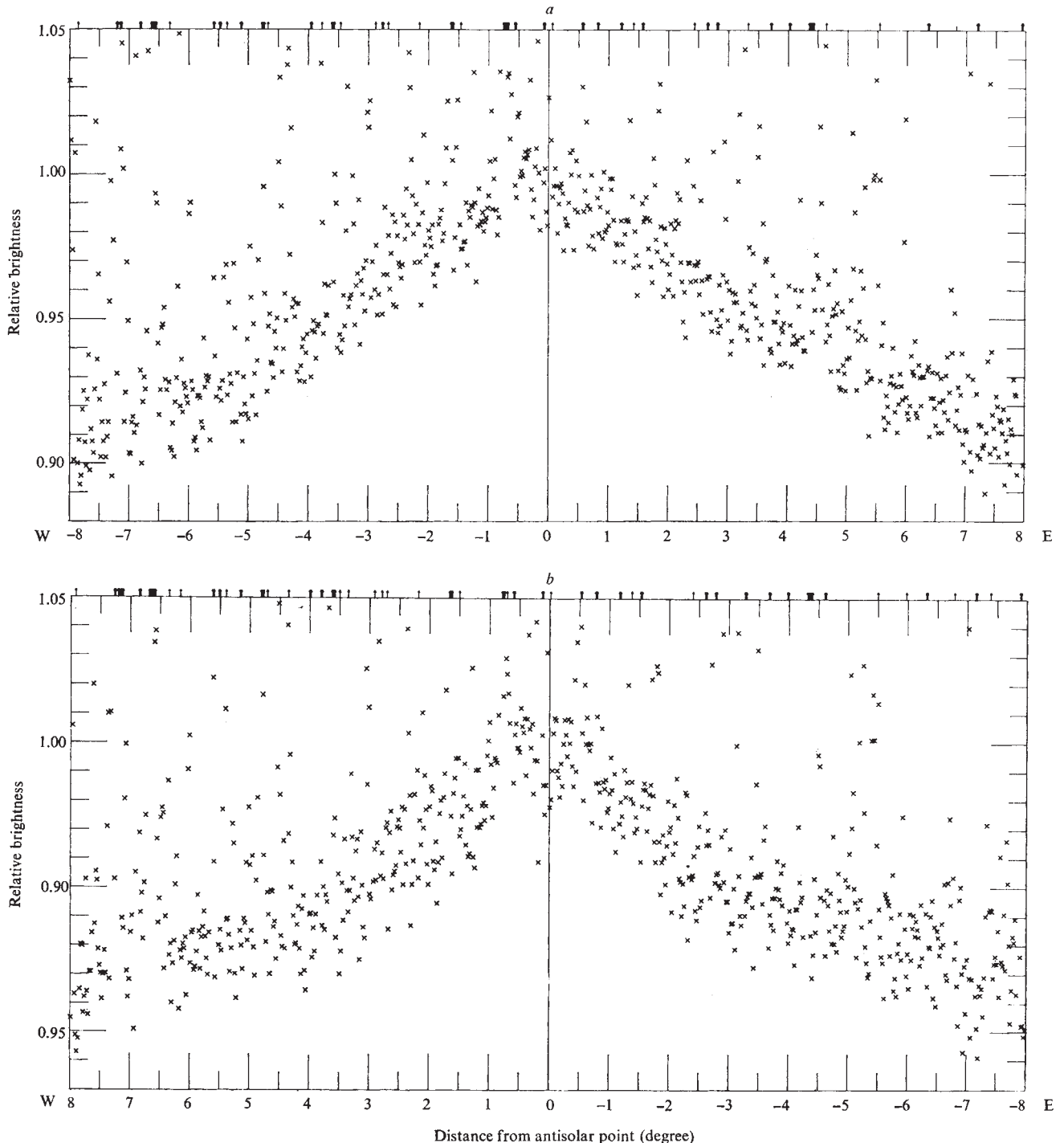


Fig. 2 Right ascension scans of the Gegenschein brightness at a constant declination of $15^\circ 26'$ and fixed hour angles (a) 2.5 h east and (b) 2.2 h west made on February 7, 1970. The centre of the Gegenschein (that is, the antisolar point) was at right ascension 9 h 22 m at local midnight. The arrows and points above the mean curves indicate high count rates due to stars. A relative brightness of 1 equals 27,000 counts.

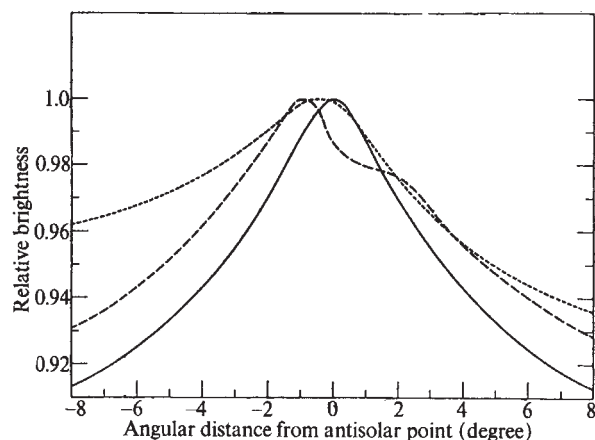


Fig. 3 The observed brightness distribution of the Gegenschein and that predicted if there were small contributions from two possible geocentric dust clouds at a zenith distance of 35° . —, Observed Gegenschein brightness; ---, plus 3% contributions from constant density geocentric dust cloud; ····, plus 5% contributions from geocentric dust cloud of density $\propto R^{-1.5}$.

umbra within 8° of the antisolar point and hence the region of perhaps the greatest concentration is invisible. Therefore the models and observations shown here are based on 35° zenith distance.

Fig. 2 shows that the observations in east and west yield identical brightness distributions. If the geocentric dust cloud contributed detectably to the brightness, however, the distributions should be asymmetrical. Fig. 3 shows the mean observed brightness curve as well as curves predicted if the geocentric dust cloud makes a contribution. (The negative direction is toward the sunlit part of the Earth.) The dashed line is the result if 3% of the Gegenschein's brightness at -8° is assumed to be produced by a geocentric dust cloud of constant density. Similarly the dotted line is the curve expected if 5% of the brightness at -8° is due to a geocentric dust cloud with density proportional to $R^{-1.5}$. The relative contribution of the assumed geocentric dust cloud is of course less at smaller distances from the antisolar point. For instance, the contribution to the curve shown for density proportional to $R^{-1.5}$ is only 2% at -2° and is of course zero between 0.0° and 0.6° .

These curves set upper limits on the possible contribution of the geocentric dust cloud since a brightness contribution greater than 3% at -8° from a constant density geocentric dust cloud would be detectable by a shift in the brightness peak while a detectable brightness asymmetry in the wings of the curve would result for a brightness contribution greater than 5% at -8° from a geocentric dust cloud with density proportional to $R^{-1.5}$.

Assuming that the dust particles in question are $10\text{ }\mu\text{m}$ in radius and have a reflectivity at opposition of 0.1, it is possible by means of a simple calculation¹⁵ to set an upper limit on the spatial density of particles in each case, remembering that 3% of the Sun's light is blocked by the Earth's shadow for the constant density cloud at -8° , while 55% is blocked for the cloud with density proportional to $R^{-1.5}$ at the same angle, using the phase function of 0.023 mag/deg and taking the Gegenschein peak brightness as 200 S_{10} (vis) units. A constant density geocentric dust cloud must have fewer than 2.3×10^{-14} particles cm^{-3} , while the maximum density just above the Earth's surface for a geocentric dust cloud for density proportional to $R^{-1.5}$ is 4.3×10^{-12} particles cm^{-3} .

Because the actual particle sizes and reflectivities are very uncertain, the above calculation of spatial density should not be considered definitive. The important point of this communication is that the hypothetical geocentric dust cloud is

undetectable in the direction opposite the Sun to a limit of a few per cent of the background sky brightness.

I thank C. L. Wolff for many useful discussions and suggestions concerning this work.

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Do Palaeogeographic Data support the Expanding Earth Hypothesis?

THE question of the contraction, stability or expansion of the Earth during geological time is one of the basic problems of the geosciences.

One of the principal arguments in support of the expanding Earth hypothesis depends on the interpretation of palaeogeographic data. By measuring the continental areas covered by seawater on palaeogeographic maps^{2,3}, Egyed noted a decrease in area covered with water towards younger geological ages. Assuming that the volume of seawater during geological time is constant or slowly growing, the decrease in the area covered with water is taken to mean that the radius of the Earth increased by about 0.5 mm/year, and such a view has been generally accepted by supporters of the expanding Earth hypothesis (see, for example, ref. 4 and also ref. 5).

It seems to me that there may be an alternative explanation of the apparent decrease with time in the continental areas covered with seawater and that this explanation may turn on the method of construction of palaeogeographic maps.

In historical geology, the subdivision of periods into epochs and ages (or systems into series and stages) is usually defined by unconformities. Because the fossil and sedimentation record of the younger geological ages is comparatively complete and well preserved, the recent geological time scale and its subdivision is comparatively precise and detailed. Because of epeirogenic and orogenic unrest, isostatic imbalance and the other causes of instability within the continental crust, together with eustatic and other changes of sea level, the chance of transgression or regression over a particular continental block will increase with the time interval considered.

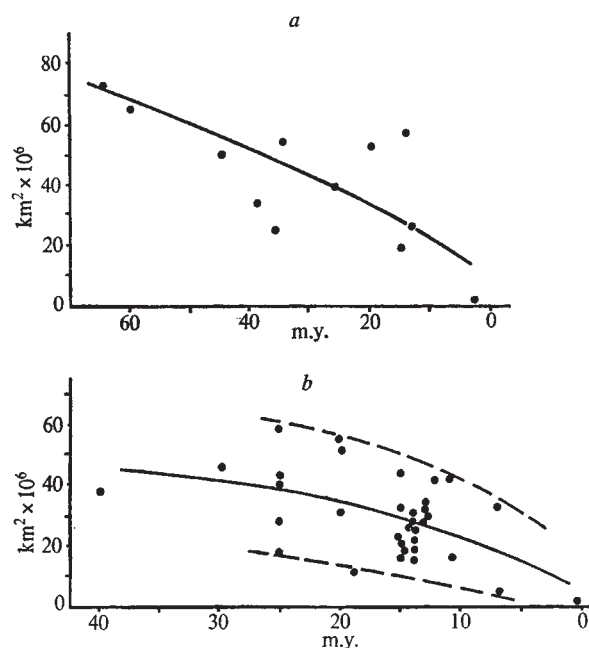


Fig. 1 Plot of continental areas covered by seawater versus the length of the time intervals represented by the palaeogeographic maps of Strakhov² (a) and Termier and Termier³ (b). The areas covered by seawater are taken from the Egyed¹ tables and corrected by 12×10^6 km² for Cambrian and Ordovician in the way suggested by Holmes⁵. The length of the time intervals represented by particular palaeogeographic maps is taken from the Geological Society Phanerozoic time scale 1964⁶.

In the reconstruction of the geological history of several continental blocks affected only by epeirogenic movements, it must be acknowledged that different blocks may be covered by seawater at slightly different times or that transgression at one end of a block may be accompanied by regression at the other. Depending on the refinement of the time scale used for this historical reconstruction, the chance that these events will be lumped together into one common transgression in the compilation of geological maps of the past will increase with the length of the time interval chosen.

If this argument is correct, there should appear to be a general decrease of the area covered by seawater with a decrease in the length of the interval chosen for the reconstruction. It follows that because of the better definition of time intervals in the more recent history of the Earth, there should be an apparent decrease with time in the continental area covered by seawater.

Plotting the Egyed data of areas covered with seawater against the length of the time intervals, the positive correlation of these two variables is indeed obvious in both sets of data (Fig. 1). As the physical time scale is not yet sufficiently precise for accurate determinations of particular stages, the graph based on the data of the Termiers is somewhat arbitrary but the general tendency is nevertheless clear. This may also account for the discrepancies between the two sets of data (see the graph of Holmes⁵): the time intervals used by Strakhov are longer than those of the Termiers, so that it is no surprise that the areas covered with water should be greater.

It is not my intention to deny the possibility that there may have been a decrease of covered areas with time but only to point out that the palaeogeographic argument in its present stage is inconclusive. The coincidence of low sea levels with ice ages and high levels with carbonate transgressions as pointed out by Fairbridge⁷ would also indicate a tectono-climatic control of the sea level rather than the expansion of the Earth. On the other hand, to maintain a balanced view, it must be noted that the small rate of expansion calculated by Egyed

cannot be rejected on the basis of palaeomagnetic data in their present state⁸.

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Basal Hot Spot on a Surge Type Glacier

DEEP temperature measurements of ice on the Fox Glacier, Yukon, indicate a zone of basal ice at the pressure melting point and the discovery of this temperate basal ice supports surge theories which require basal melting.

A surging glacier as defined by Post¹ is one which discharges an ice reservoir every 15–100+ years by means of a sudden and brief but large scale ice displacement. During a discharge, ice moves many times faster than the glacier's normal flow rate between surges. A surging glacier in its active phase can be recognized by intense crevassing, sheared margins, and large ice displacements. Meier and Post² and Post¹ have identified 204 surging glaciers in western North America.

Surge initiation mechanisms have been proposed and discussed by a number of authors and are reviewed by Robin³. Temperature instabilities resulting from deformation of basal temperature were suggested by Robin⁴ and water film instabilities at the boundary surface have been suggested by Weertman⁵. Mechanisms for raising the basal temperature commonly invoke changes in geothermal heat flow, surface accumulation, and basal sliding velocity. Glacier flow velocities are extremely sensitive to the ice temperatures near the channel boundary and in other regions of high shear stress because of the temperature dependence of the flow law for ice⁶ and the possibility of basal sliding of ice at the pressure melting point^{5,7–9}.

The purpose of our study was to discover whether surge type glaciers such as the Fox Glacier have an unusual thermal regime as required for a thermal surge mechanism. The Fox Glacier is a Type 3 surging glacier in a quiescent phase, according to the classification system of Meier and Post; it lies within the Steele Creek drainage basin and is in a region of intense surge activity. Fifteen other surging glaciers are reported to share the same drainage basin and several of these, including the Steele and Hodgson Glaciers, have surged within the past five years.

Deep thermal drilling on the Fox Glacier was carried out during the summers of 1969 and 1970 using specially designed electrically powered, cable suspended probes by H. W. C. Aamot. The design was adapted from Aamot's earlier model¹⁰ with an added provision for line heating to reduce hole closure, a severe problem when drilling cold ice. Thermistors were used to determine englacial temperatures and construction of multiconductor thermistor cables was based on standard procedures¹¹. A quartz thermometer was used to calibrate the thermistors in $3.00 \pm 0.01^\circ \text{C}$ intervals over the range -12.00°C to 0.00°C . The 1969 field measurements of thermistor resistance were performed with a Wheatstone bridge to within

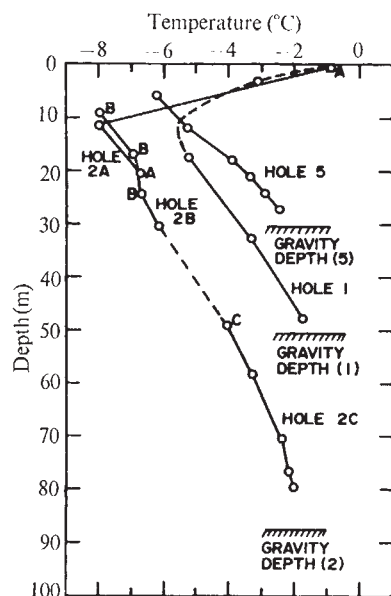


Fig. 1 Ice temperature profiles for drilling sites 1, 2 and 5. Holes 2A, 2B, 2C are different holes at the same site. Predicted ice thicknesses as determined by a gravity survey are indicated.

Table 1 Final Ice Temperature Measurements for Fox Glacier

Location	Depth (m)	Temperature (°C)	Location	Depth (m)	Temperature (°C)
Hole 1 (completed)	3.1	-3.12	Hole 4	3.1	-5.12
	17.4	-5.23		5.5	-6.55
	32.6	-3.32		8.5	-6.74
	48.0	-1.77		14.6	-6.12
Hole 2A	0.6	-0.87	Hole 5	5.8	-6.22
	11.3	-7.98	(completed)	11.9	-5.28
	20.4	-6.73		18.0	-3.93
				21.0	-3.36
Hole 2B	9.1	-7.94		24.1	-2.90
	16.8	-6.92		27.1	-2.48
	24.4	-6.70			
	30.5	-6.14	Hole 6 (completed)	22.3	-2.97
				26.8	-2.46
Hole 2C (near base?)	49.1	-4.08		29.8	-1.82
	58.2	-3.29		31.4	-1.67
	70.4	-2.40			
	76.5	-2.18	Hole 7 (near base?)	52.4	-1.58
	79.6	-2.02		62.2	-1.07
				68.3	-0.71
Hole 3 (near crevasse)	7.3	-1.82		74.4	-0.07
	14.9	-0.44		80.5	-0.19
	22.6	-0.19		84.1	-0.04
	30.2	-1.28			

Holes which are thought to be at or very near the glacier bed are marked "completed"; those which are probably close to the glacier bed are marked "near base". Hole 7 shows the presence of basal ice at the pressure melting point.

give a reliable estimate of mean annual air temperature at the surface¹⁴. It is therefore possible in principle to calculate the atmospheric lapse rate from 10 m temperature and elevation data. The lapse rate would correspond to the slope of our linear fitting function which was found to be $-10.6^{\circ}\text{C km}^{-1}$ compared with the value of $-16.0^{\circ}\text{C km}^{-1}$ found in the St Elias by Drew and Mellor¹⁵ and $-14.0^{\circ}\text{C km}^{-1}$ for the same latitude (61°N) in Greenland determined by Mock and Weeks¹⁶. Summer environmental lapse rates determined by Marcus¹⁷ for the Kluane Lake area on the eastern slope of the St Elias using radiosonde data from a station in Whitehorse give values ranging from $-6.5^{\circ}\text{C km}^{-1}$ to $-8.4^{\circ}\text{C km}^{-1}$.

None of the stations used in the lapse rate calculation were in the dry snow or percolation facies and the estimate is there-

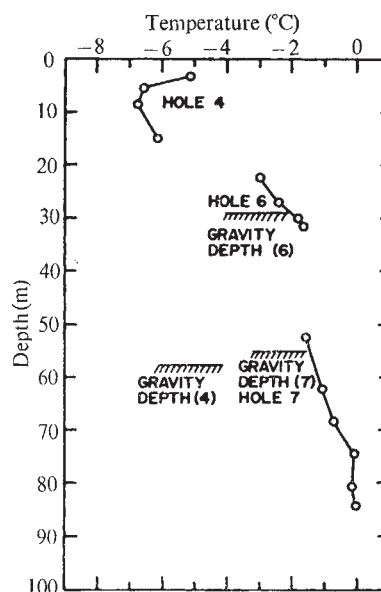


Fig. 2 Ice temperature profiles for drilling sites 4, 6, and 7. For holes 6 and 7 the actual drilled depths exceeded the gravity depth estimation. The profile for hole 7 shows a 10 m thickness of ice at the pressure melting point.

$\pm 0.15\%$ or to within $\pm 0.03^{\circ}\text{C}$ over the range of resistances encountered. Errors caused by self heating were kept well below the errors in resistance measurement. The 1970 resistance measurements were made with a digital multimeter (Fluke '8100A') to the same accuracy as the 1969 measurements. Temperatures measured one year after completion of a hole show that ice temperatures are near equilibrium after 25 d. It is therefore believed that measured temperatures are correct to within $\pm 0.1^{\circ}\text{C}$. Full details concerning the instrumentation, field procedure, and the 1969 temperature measurements are reported by Classen¹².

Thermal drilling in 1969 produced seven instrumented holes at six locations on the glacier; three holes apparently reached the glacier bed. Temperate ice was never encountered, but the data suggested the existence of two regions of temperate basal ice. Holes 2C and 7, completed in 1970, were at the sites of the predicted hot spots. Although neither of these holes is believed to have reached bedrock, a layer of warm ice was found in hole 7. Table 1 gives the final temperatures and depths for each hole; Figs. 1 and 2 present the temperature profiles together with gravity depth estimates¹³; drilling sites are indicated in Fig. 3. In every case except hole 4, the final temperatures were measured > 25 d after placement and can be considered to be at equilibrium to within 0.1°C . Temperatures for hole 4 were corrected to equilibrium values by adding -0.2°C to the measured temperatures. Data from hole 3 were discounted because they were strongly influenced by a nearby crevasse.

Holes which were considered to be at or near bedrock are so indicated in Table 1. The decision that certain probes reached the bottom was based on changes of drilling rate and compatibility with gravity depths. Incomplete holes are accounted for by hole closure or technical failure.

The temperature data from hole 7 indicate a 10 m layer of ice at the pressure melting point. The warm layer could be of greater thickness because this hole was not completed due to hole closure. It is interesting that a marked reduction in drilling rate occurred as the probe entered the temperate ice possibly because of dirt within the warm ice.

Ten metre temperatures from Figs. 1 and 2 were least squares fitted to a linear function of evaluation in order to allow a rough prediction of the 10 m ice temperatures for the entire glacier. Temperatures measured at a 10 m depth in the dry snow and upper percolation facies of glaciers and ice sheets

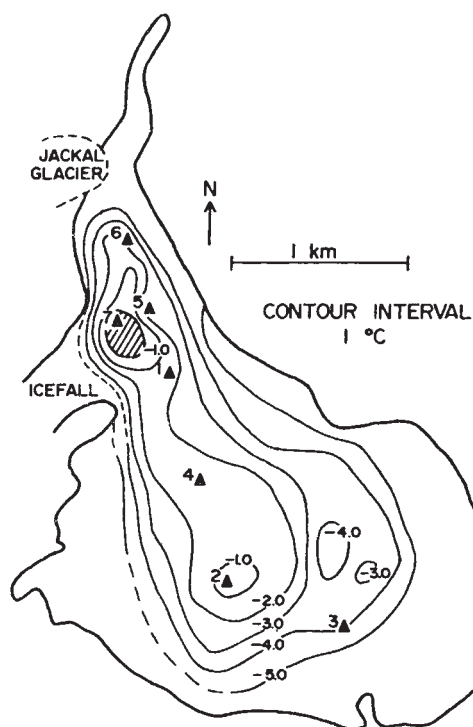


Fig. 3 Contour map of predicted basal ice temperatures for the Fox Glacier. Solid triangles are drilling sites. The shaded zone is the basal hot spot discovered at site 7. The contour interval is in $^{\circ}\text{C}$.

fore subject to errors. For regions below the upper percolation facies the percolation of meltwater causes a downward transport of latent heat which tends to increase the 10 m snow temperature. There is no permanent snow cover in the ablation region, and downward percolation of meltwater through the ice is negligible for non-temperate glaciers. The surface ice temperature cannot exceed 0°C so the annual temperature wave is clipped and the 10 m ice temperatures are therefore less than the mean annual temperature.

An estimate of geothermal heat flow was determined from the deep temperature gradient in hole 2C. The thermal conductivity for ice was taken as $k = 5.44 \times 10^{-3}$ calories $(\text{cm s } ^{\circ}\text{C})^{-1}$ and the resulting value for geothermal flux was $Q = 2.34$ $\mu\text{calories cm}^{-2} \text{ s}^{-1}$, about twice the worldwide average. The interpretation of this temperature gradient as a geothermal gradient is suspect because of the shallowness of this hole and the large but uncorrected topographic distortion. Estimates from the shallower holes gave even higher values for the heat flow.

A basal temperature model (Fig. 3) was constructed by extrapolating predicted 10 m temperatures at the gravity depth stations of Crossley and Clarke¹³ to the glacier bed. Linear fits to the temperature gradients from Figs. 1 and 2 were used in the extrapolation. No other hot spots are predicted by this simple model, and the detected hot spot is shown to be of small extent, but because of the uncertainties in gravity depth determination, the glacier could be thicker than suggested, allowing the possibility of a more extensive zone of warm ice and other undiscovered hot spots. Weertman¹⁸ has shown that basal water will transport heat to regions which are not at the pressure melting point, and the result is an enlarged melting zone. The true extent of the temperate basal ice is therefore unknown.

The surge behaviour of the Fox Glacier must surely relate to its remarkable thermal regime, but whether the observed hot spot is the cause of the Fox Glacier's surge activity or a consequence of its most recent surge is still open to question. It is significant that the Fox Glacier satisfies the essential requirement of thermal surge theories.

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Radiometric Age Determinations on Pliocene/Pleistocene Formations in the Lower Omo Basin, Ethiopia

THE potassium-argon ages presented here were obtained during 1966 to 1969 in order to provide an absolute time scale for the stratigraphic work by the international Omo Research Expedition in the Pliocene/Pleistocene formations (unpublished work of F. H. B., J. de Heinzelin and F. C. Howell) in south-west Ethiopia. Although some of these dates are not new¹⁻³, most of the analytical procedures and data have not been presented. We also present a list of fossil localities recorded by the University of Chicago contingent of the expedition within the Shungura Formation. Preliminary descriptions of the Hominidae have been published already^{3,4}.

The K-Ar age of the Mursi Formation was obtained from a whole-rock sample of the slightly altered basalt flow which has been designated member IV by Butzer and Thurber⁵. Fitch and Miller (personal communication) have since made further K-Ar determinations on this basalt with comparable results. Immediately overlying member IV of the Mursi Formation is a lapilli tuff which forms the base of the Nkalabong Formation. Feldspar separated from individual lapilli from this tuff was dated by Fitch and Miller² using the technique of neutron irradiation total degassing. Their results are included in Table 1 for convenience.

Two different materials, whole-rock basalt and rhyolitic glass, were analysed to date the Usno Formation. The basalt, which is very slightly weathered, is a thin flow lying near the base of the formation. The rhyolitic glass was obtained from a tuff bed at the base of the Three Tuffs Sequence underlying the fossiliferous Brown Sands Sequence⁶.

Mineral concentrates of anorthoclase feldspar were the only materials used in dating the Shungura Formation. The feldspar was primarily from large rounded cobbles of rhyolitic

pumice in the major tuffs defining the bases of the members in this formation (unpublished work of F. H. B., J. de Heinzelin and F. C. Howell). The outer surfaces of the pumice cobbles were removed and the feldspar phenocrysts were separated from the cores to prevent contamination by non-volcanic materials. In addition, only coarse grain sizes were used in the initial separations to prevent contamination because most of the feldspar grains in the tuff matrix are the size of medium sand to silt. A sandy tuffaceous horizon 20–50 cm thick, designated tuff *E_i*, occurs between tuff *R* and tuff *E*. It contains large (3–5 mm) crystals of anorthoclase feldspar which were individually collected in the field. These crystals were examined under a binocular microscope and all turbid and otherwise altered grains were removed. The remaining crystals were crushed and prepared in the same manner as the feldspar samples extracted from pumice fragments.

All the feldspar and glass concentrates were purified by standard heavy-liquid and magnetic techniques. Each sample was leached slightly in hydrofluoric and hydrochloric acid to remove surface impurities. Though analysed on a whole-rock basis, the basalt samples were crushed and leached with hydrochloric acid to remove alteration products such as carbonates.

The samples in Table 1 with *KA* designations were analysed by both of us in the laboratory of G. H. Curtis at the University of California, Berkeley, and those with *LKA* designations were analysed by one of us (K. R. L.) in the laboratory at the Lamont–Doherty Geological Observatory in Palisades, New York. Argon was extracted under ultra-high vacuum by fusion in an induction furnace. The gas from samples analysed in the Berkeley laboratory was cleaned in a two-stage process using a hot copper and copper oxide and titanium getters. The gas from samples analysed in the Lamont–Doherty laboratory was cleaned in a one-stage process using two titanium getters heated to 400° and 800° C. The cleaned argon was analysed statically on Reynolds-type, 60°, 4.5 inch radius mass spectrometers. Potassium was analysed by flame photometry using lithium buffers and potassium external standards.

The constants used in the calculations are $\lambda_e = 0.584 \times 10^{-6}$, $\lambda_\beta = 4.72 \times 10^{-10}$, and $^{40}\text{K}/\text{K}_{\text{total}} = 1.19 \times 10^{-4}$ mole %.

An estimate of the precision (analytical reproducibility) of each K–Ar analysis is given as a $\pm$ value in the eighth column of Table 1. These values were derived using a graphical

representation of the procedure described by Lipson⁷. The principal factor contributing to this error is the percentage atmospheric argon correction for each analysis (sixth column, Table 1). For atmospheric argon corrections up to about 70% the estimated analytical error is between 3 and 5% of the age and for corrections around 90% the error is about $\pm 20\%$ of the age. This procedure does not yield a statistically meaningful estimate of the precision, for which replicate analyses on each sample are needed.

The resolution of the K–Ar age determinations (the ability to distinguish separate, closely spaced geological events) can be evaluated on the basis of (1) the percentage estimated analytical error for each experiment; (2) the agreement of results from separate samples of the same tuff bed; and (3) the agreement of results with stratigraphic relationships. Considering all these factors we estimate the resolution to be between 5 and 10% of all ages reported here.

The only checks available are the four analyses of the mineral concentrates from tuff *D*. The average age from the two Lamont–Doherty analyses (*LKA*-22 and *LKA*-23) is 2.23 m.y. which is about 10% younger than the average age, 2.45 m.y., from the two Berkeley analyses (*KA*-2067 and *KA*-2176). With these limited data it is impossible to attribute this discrepancy solely to errors resulting from differences in procedures in the analytical laboratories. Consequently, all four ages are combined yielding an average age of 2.35 ± 0.12 for tuff *D*.

Although all the K–Ar ages lie in proper stratigraphic order, the apparent differences in age between any two adjacent dated horizons are generally too small (in most cases less than 10% of the age) to be statistically meaningful. Even with this minor limitation gross parameters such as rates of deposition can be estimated using these data for various parts of the sequence. Further work to verify and refine our results is in progress.

An inventory of fossil localities investigated by the University of Chicago contingent of the expedition is given in Table 2. The localities are separated stratigraphically and include nearly all sites designated in the 1968 and 1969 field seasons. Where the fossils were recovered from within a tuff, the stratigraphic position is given as tuff *D*, and so on. In all other cases, localities from between tuffs are lumped together and assigned to the appropriate member; we have made no attempt here to separate them with regard to their position within

Table 1 Analytical Data for K–Ar Age Determinations

Stratigraphic unit	Sample number	Material analysed	Sample weight (g)	K ⁺ (%)	AIR ⁴⁰ Ar (%)	Calculated age (m.y.)	Average calculated age (m.y.)
Shungura Formation							
Tuff <i>I</i> ₂	{ <i>KA</i> -2187 <i>KA</i> -2085	Feldspar	5.2886	5.182	54.8	1.81 ± 0.09	1.84 ± 0.09
		Feldspar	5.0010	5.111	—	1.87 ± 0.09	
Tuff <i>G</i>	<i>LKA</i> -9	Feldspar	1.9868	5.899	59.3	1.93 ± 0.10	1.93 ± 0.10
Tuff <i>F</i>	{ <i>LKA</i> -11 <i>LKA</i> -21	Feldspar	1.6682	6.101	66.4	1.99 ± 0.10	2.04 ± 0.10
		Feldspar	2.0586	6.101	48.2	2.06 ± 0.10	
Tuff <i>E</i> _i	<i>LKA</i> -14	Feldspar	2.3350	5.010	49.1	2.12 ± 0.11	2.12 ± 0.11
Tuff <i>D</i>	{ <i>LKA</i> -23 <i>LKA</i> -22 <i>KA</i> -2176	Feldspar	1.8013	5.151	52.4	2.16 ± 0.11	2.35 ± 0.12
		Feldspar	1.6478	5.151	37.3	2.31 ± 0.11	
		Feldspar	5.0001	5.432	53.6	2.37 ± 0.12	
Tuff <i>B</i>	{ <i>KA</i> -2067 <i>KA</i> -2096	Feldspar	5.0023	5.151	51.0	2.56 ± 0.12	3.75 ± 0.20
		Feldspar	6.0050	4.625	50.3	3.75 ± 0.20	
Usno Formation							
<i>WS</i> -1 Basalt	{ <i>LKA</i> -25 <i>LKA</i> -2 <i>LKA</i> -20	Glass	0.7766	2.830	91.6	2.64 ± 0.92	2.64 ± 0.92
		Whole-rock	10.8442	0.7074	35.4	3.11 ± 0.15	
		Whole-rock	6.3263	0.7074	90.7	3.51 ± 0.70	
Nkalabong Formation							
Lapilli Tuff*	{ — —	Feldspar	—	—	67.2	3.90 ± 0.10	3.95 ± 0.11
		Feldspar	—	—	67.1	3.99 ± 0.12	
Mursi Formation							
<i>YS</i> Basalt	<i>KA</i> -2094	Whole-rock	10.1667	0.828	75.4	4.05 ± 0.20	4.05 ± 0.20

* These age determinations were made by Fitch and Miller² using the neutron irradiation total degassing technique.

Table 2 Stratigraphic Position of Fossil Localities of the Shungura Formation

K-Ar age ($\times 10^6$ yr)		Locality Nos. 1-99	Locality Nos. 100-199	Locality Nos. 200-299	Locality Nos. > 300
1.93	Member G	7*, 16, 24, 25, 29, 35, 43, 44, 48, 60-62, 67, 74*, 80, 87, 96	112	245-251	328, 336, 337, 341
	Tuff G			252, 256	
	Member F	3, 28*, 38-41, 52, 63, 65, 66, 75, 79, 89	105, 116, 131, 133, 145, 157*, 162, 181, 182, 183*, 189	205, 238*, 239-244, 253-255, 258, 259	
2.04	Tuff F			215, 257	
	Member E	4, 5/6, 10, 13, 20, 26*, 31, 49, 81-83, 84, 88, 92, 94, 97, 99	101, 102, 114, 115, 127, 128, 139, 141, 142, 146, 147, 153, 154, 156, 163, 164, 170-174, 176-179, 185-187	204, 206-210, 233-237, 260-266, 268-270	338*, 339, 340
	Tuff E		155, 165, 175	267, 271	310, 311
2.35	Member D	9*, 11, 12, 14, 19, 21, 22, 30, 36, 37, 50, 56, 64*, 72, 73, 76, 77, 90, 95, 98	100, 104, 118-123, 129, 137, 138, 148-151, 160, 161*, 166-169, 184, 190-192	211-214, 216, 217, 225-228, 230-232, 273	314, 348
	Tuff D		140, 195	224, 274, 275	342, 356
	Member C	8, 15, 17, 18, 23, 27, 32, 33, 34, 42, 45*, 46, 47, 51*, 53, 54*, 55, 57- 59, 68-70, 78, 86, 91, 93	103, 107-111, 113, 117, 124-126, 130, 132, 134, 136*, 143, 144*, 152, 158, 159, 180, 193, 194, 196-199	203, 218-223, 272, 276- 295, 296*, 297-299	300-308, 312, 313, 315- 326, 327*, 329-335, 343- 347, 349-355, 357-361, 362*, 363, 364
3.75	Tuff C				
	Member B	1, 2*			
	Tuff B				

* Sites have yielded remains of Hominidae.

a member. All sites which have yielded remains of Hominidae are starred in this table.

Six fossil localities have been deleted from this table—localities 135, 188 and 309 were eliminated because of structural complexity in the locality which may result in a mixed fossil assemblage; localities 85, 106, and 229 were eliminated because their stratigraphic positions are uncertain.

The chief conclusions to be drawn from the present K-Ar data are: (1) the Omo Group and its included fauna ranges in age from about 1.8 to 4.0 m.y.; (2) the base of the Usno Formation is roughly equivalent in age to tuff C of the Shungura Formation; and (3) the Nkalabong and Mursi Formations are probably slightly older than the oldest exposed deposits of the Shungura Formation.

Brown¹ and de Heinzelin and F. C. Howell (unpublished work) have outlined the relationships of the earlier Pliocene-Pleistocene Formations in the Omo Valley to the fossiliferous deposits at Olduvai and at Kanapoi. Those at Olduvai (<1.9 m.y.) are younger than most of the Omo sequence and those of Kanapoi (>2.5 m.y.) probably correlate with the older parts of the Shungura Formation (members A, B, C), but on faunal grounds they may well be older⁸. Leakey⁹ has described fossiliferous exposures east of Lake Rudolf which have been dated by Fitch and Miller¹⁰ at 2.61 m.y., making them correlative with the older members of the Shungura Formation, and about the same age as the most fossiliferous portion of the Usno Formation.

As radiometric dates are obtained from more fossil localities in East Africa, knowledge of the temporal and spatial distribution of vertebrate taxa becomes clearer and more secure.

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Mössbauer Spectroscopy of Ancient Greek Pottery

THE analytical value of Mössbauer spectroscopy has been demonstrated in several studies of clay minerals¹ and mineral rocks² containing iron, and so Mössbauer spectra of pottery, which is made of baked clay, should provide useful information for the assessment of archaeological findings.

A study of mediaeval pottery from Essex in Britain has been reported already³, and we describe here an exploratory Mössbauer study of ancient Greek pottery obtained from recent

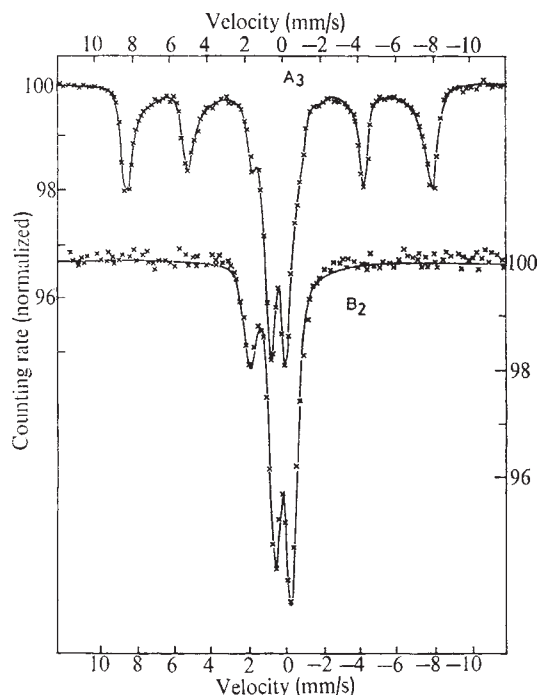


Fig. 1 Representative Mössbauer spectra from two groups of ancient ware from the Vitsa Zgorion (north-western Greece) excavation. *A*, Local hand made ware about 800 bc; *B*, Korinthian like ware about 750 bc.

excavations of a cemetery near Vitsa in the region of Zagori (Ioannina) in north-western Greece.

The observed differences in the Mössbauer spectra correlate well with a classification of the samples based on archaeological criteria. Variations in the parameters of the Mössbauer spectra, originating from a difference in the oxidation state of iron or the symmetry around it, may be due to such factors as: (i) the constitution of the original clay material; (ii) the constitution of tempering additives, and (iii) the firing conditions (temperature and oxidizing or reducing atmosphere).

About 200 mg of material was scraped from the interior surface of the pottery (complete or sherd), and a conventional Mössbauer spectrometer was used to record the spectra of the samples at room temperature. Sixteen samples, classified into four groups for archaeological reasons, were examined and found to have a resonance absorption of the order of 10%. This result is in agreement with the well known fact⁴ that ancient Greek pottery contains more iron, in the body as well as on the surface, than modern pottery.

A preliminary sorting of the samples is possible by visual examination of the spectra. Two typical spectra of samples of two different groups are shown in Fig. 1. Closer examination shows that the spectra can be analysed as a superposition of components of paramagnetic ferrous and ferric ions similar to those found in silicate minerals (unpublished work of A. Nobeli, N. Yassoglou, A. K. and A. S.) and ferromagnetic iron oxides (Fe_2O_3 or Fe_3O_4).

The correlation of the classifications based on archaeological evidence and visual appearance on the one hand, and Mössbauer parameters on the other hand, is summarized in Table 1. The values of Mössbauer parameters (intensity, isomer shift, quadrupole splitting and magnetic field) have small dispersion within each group, but for several of them there are appreciable differences between groups. One sample, originally assigned to group *B* because of the appearance of the clay, had Mössbauer parameters markedly different from those of the other samples in the group. Because this sample was obtained from a small sherd, it probably did not belong to group *B* and has been excluded from Table 1. Sherds of the same appearance

Table 1 Correlation of Archaeological Evidence and Mössbauer Parameters for Samples of Ancient Greek Pottery

Mössbauer parameters	Grouping by archaeological evidence			
	<i>A</i> (3 samples)	<i>B</i> (4 samples)	<i>C</i> (3 samples)	<i>D</i> (4 samples)
Non-magnetic part				
Fe^{3+} ; ΔQ	0.8	0.7–0.8	0.8	0.7–0.9
i.s.	0.3–0.4	0.3–0.5	0.25–0.35	0.1–0.3
Fe^{2+} ; ΔQ	—	(<i>B'</i>) 2.1–2.3	(<i>B''</i>) —	—
i.s.	—	0.9–1.1	—	—
Magnetic part				
ΔQ	0.4–0.6	—	0.15–0.25	0.2–0.4
i.s.	0.5–0.6	—	0.3–0.5	0.15–0.3
H_{eff}	500–510	—	480–510	475–505
I_{tot}	8–10	6.9–7.5	4.0–6.5	4.5–5.0
I_M/I_{tot}	0.4	0	0.1–0.2	0.2–0.3

ΔQ , Quadrupole splitting in mm/s; i.s., isomer shift in mm/s (relative to iron metal); H_{eff} effective magnetic field in kG; I_{tot} , total absorption in percentage per 100 mg of material; I_M/I_{tot} , ratio of absorption of magnetic part to total; *A*, local hand made ware, orange red coarse clay with yellow particles, geometric period about 800 bc; *B*, Korinthian or Korinthian like ware, fine pale clay; *B'*, Korinthian like, geometric period about 750 bc; *B''*, Korinthian oriental like period about 600 bc; *C*, sherds of probable attikizing type, pale brown red fine clay with black glaze on exterior surface, classical period about 500 bc; *D*, local hand made ware, brown red clay, geometric period about 800 bc.

have widely different mineralogical constitution⁵, and it is also worth noting that the separation of the samples of group *B* in two subgroups *B'* and *B''* depending on the date and style of the ware is clearly shown in the Mössbauer spectra.

These results demonstrate that the Mössbauer spectra of ancient Greek pottery are sufficiently different to be used at least as supporting evidence for the classification of the findings from an archaeological site. This requires a systematic study of the characteristic Mössbauer pattern of the principal types of ancient Greek pottery with regard to source of the clay material and firing techniques.

We thank Dr A. G. Maddock for drawing our attention to the possibility of applying Mössbauer spectroscopy to ancient pottery.

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Photo-oxidation of Atmospheric SO_2

THE photo-oxidation of sulphur dioxide to sulphuric acid in the atmosphere is relevant both to the formation of aerosols in polluted air and to the life cycle of sulphur compounds in the atmosphere.

We have recently reported¹ some experimental values for the rate of photo-oxidation of SO_2 in a 220 l. chamber exposed to natural sunlight. A radioactive tracer method was developed to measure the very small quantities of sulphuric acid aerosol

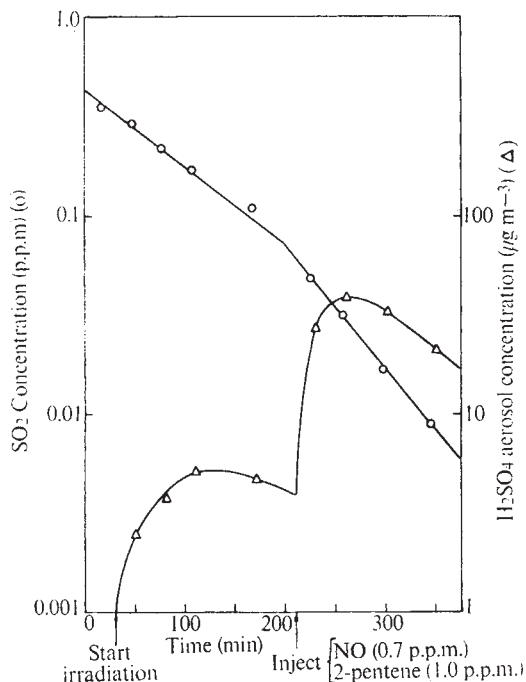


Fig. 1 Aerosol formation and SO_2 decay during the photo-oxidation of SO_2 in the presence of nitric oxide and 2-pentene.

produced when SO_2 is oxidized at atmospheric concentrations. The sensitivity was such that conversions of the order of 0.02% could be detected at SO_2 concentrations less than 0.1 p.p.m. The observed conversion rates of SO_2 , based on the rate of aerosol formation, ranged between 0.04 and 0.65% h^{-1} for SO_2 concentrations of 0.07–0.7 p.p.m. The higher values are considerably larger than those reported by Gerhard and Johnstone² (0.1–0.2% h^{-1}) which have been widely quoted for the rate of photochemical oxidation of SO_2 in the atmosphere. It was thought that this discrepancy could be accounted for in terms of the relatively rapid diffusion of primary sulphuric acid nuclei to the walls of the small (8 l.) reaction vessel².

It was also suggested that the rates are affected by participation of trace materials in the air¹. The rate of photochemical aerosol production, measured by light scattering techniques, is known to be accelerated when nitric oxide and olefinic hydrocarbons are present with SO_2 (ref. 3). This aerosol has been shown to consist almost entirely of sulphuric acid droplets^{4,5} at intermediate or high relative humidities ($\geq 50\%$), but the only experimental determination of the mass concentration of sulphuric acid aerosol produced in this system⁶ shows a 7 to 10% conversion of SO_2 to sulphate aerosol in a 2 h irradiation of mixtures of propylene (2 p.p.m.), NO (0.5 p.p.m.) and SO_2 (0.45–1.1 p.p.m.). Analysis of the chamber air in our experiments showed that hydrocarbon compounds and nitrogen oxides were present in small amounts (0.1–0.5 p.p.m. and <0.005 p.p.m. respectively) and it was suggested that the presence of these substances may be responsible for the high observed photo-oxidation rates¹.

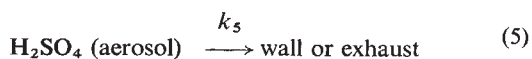
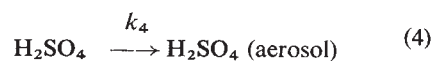
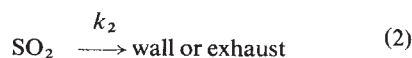
We have now obtained experimental evidence which strongly supports the proposal that low concentration olefinic hydrocarbons and nitric oxide can strongly influence the rate of photo-oxidation of SO_2 in air. Mixtures containing sub-p.p.m. quantities of SO_2 , nitric oxide and *cis*-2-pentene in air were photolysed in the 220 l. aluminium chamber¹. A medium pressure mercury arc (Englehard-Hanovia Ltd, 'UV 550') was used as a light source and the light entered the chamber through a 1/16 inch 'Pyrex' glass window which replaced the original 'Perspex' window and filtered out the high energy radiation of wavelength less than 2900 Å.

Aliquots of gaseous sulphur dioxide $^{35}\text{SO}_2$ and nitric oxide (Air Products Ltd, 'CP' grade) were measured into glass

injection cells and flushed into the chamber in a stream of pure nitrogen. Liquid *cis*-2-pentene was injected from a microlitre syringe and allowed to vaporize into the chamber.

Analyses for gaseous SO_2 and sulphuric acid aerosol were made periodically using the radiochemical techniques described previously¹. The variation with time of the concentrations of SO_2 and aerosol in the photolysis of SO_2 (0.30 p.p.m.), NO (0.73 p.p.m.) and *cis*-2-pentene (0.98 p.p.m.) is shown in Fig. 1; the formation of small amounts of sulphuric acid aerosol on irradiation can be clearly seen. The aerosol concentration increased to a maximum of about $5.2 \mu\text{g m}^{-3}$ and then declined. When the nitric oxide and olefin were introduced there was a dramatic rise in the rate of aerosol formation and a second maximum of about $40 \mu\text{g m}^{-3}$ was reached after 11 min.

Rate constants for sulphuric acid formation were calculated from the data of several experiments assuming the following reaction path



Reaction (1) represents the oxidation of SO_2 which may be catalysed by light or by oxidants formed in the NO-olefin photochemical system. Reactions (3) and (4) are very rapid^{7,8} and it follows that the rate of increase in concentration of aerosol is given by

$$\frac{d[\text{H}_2\text{SO}_4 \text{ aerosol}]}{dt} = k_1[\text{SO}_2]^n - k_5[\text{H}_2\text{SO}_4]$$

If it is assumed that $n=1$ (that is, reaction (1) is first order with respect to SO_2), it can be shown that the maximum concentration of aerosol is given by

$$[\text{H}_2\text{SO}_4 \text{ aerosol}]_{\text{max}} = \frac{k_1}{k_5} [\text{SO}_2]_0 \exp(-(k_1 + k_2)t_{\text{max}}) \quad (6)$$

where t_{max} is the time to reach maximum aerosol concentration and $[\text{SO}_2]_0$ the initial sulphur dioxide concentration.

Table 1 Aerosol Production in SO_2 -NO-*cis*-2-pentene Photolysis

Experiment No.	Initial concentrations (p.p.m.) SO_2	NO	2-pentene	k_1 $\text{h}^{-1} \times 10^2$
1	1.00	—	—	0.20
2	0.30	—	—	0.29
3	0.26	—	—	0.18
4	0.14	—	—	0.17
5	0.07	0.73	0.99	9.3
6	0.06	0.34	0.52	8.3
7	0.07	0.07	0.21	5.1
8	0.05	0.03	0.10	2.5
9	0.11	—	1.03	0.81
10	0.20	0.35	—	0.53
11	0.25	0.35	0.52	11.1
12	0.25	0.35	0.10	8.6
13	0.22	0.05	0.52	4.1

Table 1 shows the oxidation rate constants k_1 , calculated by equation (6) for various initial concentrations of SO_2 , NO and *cis*-2-pentene. The values were determined on the assumption that the removal of aerosol (k_5) is determined only by the flow rate of flushing air and therefore represents lower limits of k_1 . The constant k_1 for SO_2 alone was essentially independent

of the SO_2 concentration in the range 0.14–1.0 p.p.m., indicating a first order reaction with a rate constant of $2 \times 10^{-3} \text{ h}^{-1}$. The photo-oxidation rate increased markedly when NO and *cis*-2-pentene were present; the rate constant k_1 derived in experiment 8, with NO=0.03 p.p.m. and *cis*-2-pentene=0.1 p.p.m., is greater by a factor of 10, for example, than for SO_2 alone. The rate was always significantly greater in the presence of the two additives than when either NO (0.35 p.p.m., experiment 10) or *cis*-2-pentene (1.03 p.p.m., experiment 9) alone was added. The higher rates in experiments 9 and 10, compared with those for SO_2 alone, may be due to the presence of residual traces of NO and olefinic hydrocarbons in the chamber air. Comparison of experiments 6 and 11 shows that k_1 only increased slightly with a four-fold increase in the SO_2 concentration. In the last three experiments (Table 1) the NO and *cis*-2-pentene levels were varied independently showing that considerable reductions in the concentrations of *cis*-2-pentene and NO (by factors of 5 and 7 respectively) do not cause a large drop in the rate. The sensitized photo-oxidation is evidently not particularly affected by the changing levels of these additives in the concentration range used.

It is most likely that the enhanced oxidation rate of SO_2 to H_2SO_4 aerosol is caused by oxidants produced in the NO-olefin photochemical system. The initiating reaction in this system is the photolysis of NO_2 (produced by reaction of either O_2 or O_3 with NO) to give an oxygen atom. A complex series of reactions involving O_3 , O atoms and the hydrocarbons leads to the formation of the well known "photochemical smog". The nature of the oxidant(s) responsible for the oxidation of SO_2 in this system has not been established. At low concentration, O_3 does not react rapidly with SO_2 (ref. 9) but such species as peroxy radicals or peroxyacyl nitrates, which are known to be formed in this system, seem likely candidates.

The application of the rates in Table 1 to atmospheric conditions demands a knowledge of the light intensity used in our experiments relative to sunlight and also an estimate of the average levels of oxides of nitrogen ($\text{NO} + \text{NO}_2$) and reactive hydrocarbons present in urban environs.

A measure of the average light intensity in the chamber was obtained from the rate of photolysis of pure NO_2 in an atmosphere of N_2 (ref. 10). This gave a value for the rate of absorption of light by NO_2 (ϕk_a) of 5.0 h^{-1} , which may be compared with $\phi k_a = 20 \text{ h}^{-1}$ calculated for sunlight with a solar zenith angle (Z) of 45° (ref. 11). If it is assumed that NO_2 is the primary absorber responsible for the oxidation of SO_2 , then the effective light intensity in the chamber was approximately one quarter that of sunlight when $Z = 45^\circ$.

The average level of $\text{NO} + \text{NO}_2$ found in London was about 0.05 p.p.m. in 1960 (ref. 12); other urban values outside Los Angeles vary from about 0.03 to 0.1 p.p.m.¹³. A detailed analysis of the gaseous hydrocarbon content of the air is necessary before its reactivity can be ascertained because hydrocarbons vary in their ability to produce oxidants with NO_2 . It has, however, been suggested that approximately one third of the hydrocarbons produced by urban sources are photochemically active¹⁴. Samples taken in 1969 at AERE, Harwell, showed that the levels of gaseous hydrocarbons other than methane varied from <0.1 to 0.5 p.p.m.¹. In terms of reactive hydrocarbons this indicates that levels between <0.03 and 0.15 p.p.m. can be expected.

The rate constant for oxidation of SO_2 in the experimental chamber is $2.5 \times 10^{-2} \text{ h}^{-1}$ with concentrations of NO and *cis*-2-pentene of 0.03 and 0.1 p.p.m. respectively. If this value is scaled up to account for the reduced light intensity, a conversion rate of about $10\% \text{ h}^{-1}$ can be expected in the atmosphere in London. This may be a rather high estimate, because *cis*-2-pentene is a very reactive olefin, but it provides convincing evidence that the values obtained in our earlier experiments for the oxidation of SO_2 ($0.65\% \text{ h}^{-1}$ (ref. 1)) are more appropriate to the atmospheric situation than the generally accepted value of 0.1 to $0.2\% \text{ h}^{-1}$ (ref. 2). At a rate of $0.65\% \text{ h}^{-1}$, significant quantities of the sulphate aerosol

responsible for reducing atmospheric visibility will be formed photochemically.

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Energy Transfer from Excited Gaseous Molecules to Solids

PREVIOUSLY¹ we reported experiments suggesting that vibrationally excited molecules are able to induce electron transitions in solids. In brief, the experimental method is as follows: a "molecular beam" is made to strike a hot filament and then to impinge on a detector. The detector is a thin film of zinc oxide obtained by oxidation in air of a layer of zinc prepared by vacuum evaporation². The electronic transitions in the zinc oxide are afterwards observed by measuring the thermostimulated electron emission of the sample in a proportional counter³. This technique is somewhat similar to that used by Dorrestein⁴ for the detection of excited atoms and radicals, but seems to be more sensitive because it can be used to integrate small excitations during long periods.

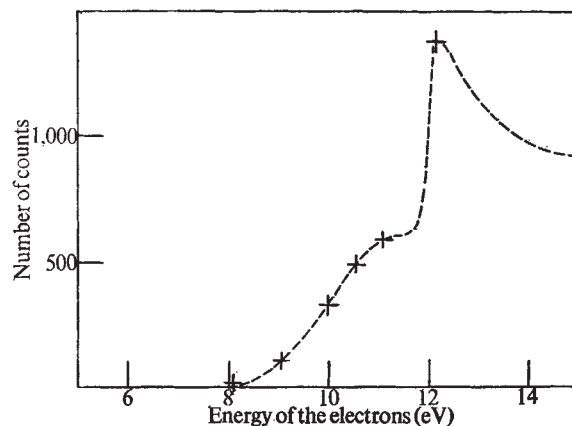


Fig. 1 Number of thermostimulated electrons emitted by the zinc oxide target as a function of the energy of the exciting electrons. +, Experimental points; ---, "excitation function" for nitrogen.

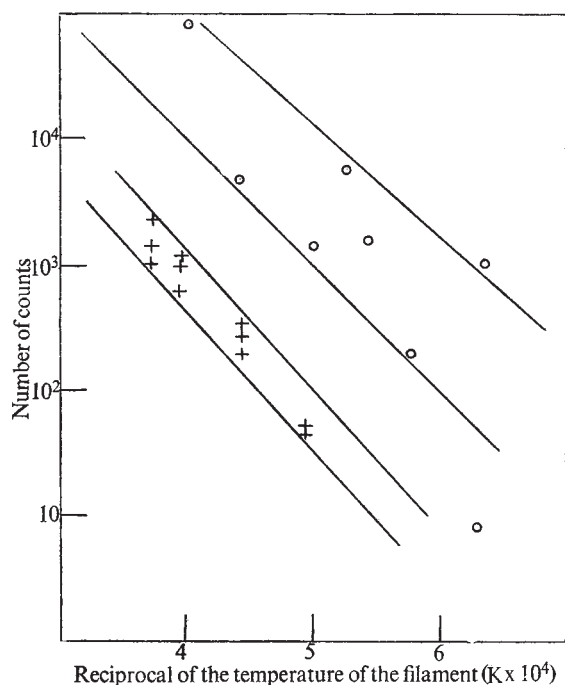


Fig. 2 Logarithm of the number of thermostimulated electrons as a function of the reciprocal temperature of the filament (K). +, Nitrogen; o, carbon dioxide.

Precautions were taken to prevent bombardment of the zinc oxide by electrons emitted from the filament; but we assumed that thermal excitation by contact with a hot filament cannot induce electronic transitions in the gas molecules being studied. In our first experiments we used a platinum filament, thus setting a limit of about 1,400° C to the working temperature. In these conditions a positive effect was easily found with carbon dioxide, but no effect could be detected with nitrogen or hydrogen. The replacement of the platinum filament by a rhenium filament has enabled us to increase the temperature to above 2,000° C, at which a positive reaction is obtained with nitrogen and hydrogen. No effect is obtained with argon. This confirms that the effect may be attributed to a gas particle and not to an excited atom emitted by the filament.

It is, however, possible that the active particles are atoms recombining at the surface of the detector, liberating the dissociation energy of the molecule. We therefore tried to excite the gas molecules by electron bombardment: no effect at all could be observed with carbon dioxide. Nitrogen gave a positive reaction (Fig. 1), but it can be seen that this "excitation function" is very similar to that obtained for the excitation of metastable states⁵. No effect could be observed below 8 eV whereas vibrational levels should be observed between 0.3 eV and 3 eV. The cross-section for slow electron excitation is known to be rather small⁶, and the electron gun used in our experiments provides negligible currents at low energy: negative results are thus not incompatible with the mechanism proposed.

A series of experiments was undertaken in an attempt to measure the "activation energy" of the process. The total number of thermostimulated electrons was recorded as a function of the temperature of the filament (measured with an optical pyrometer). All other parameters were maintained constant. Because of the technical difficulty of these experiments results are somewhat scattered. From a large number of measurements, however, the diagram in Fig. 2 can be obtained by plotting the logarithm of the total number of thermostimulated electrons as a function of the reciprocal temperature of the filament.

The results obtained, grouping various samples of zinc oxide, are contained in a sector whose axis may be used to calculate an "activation energy". This energy is found to be

42 kcalories for carbon dioxide and 51 kcalories for nitrogen. These energies are in the range of vibrational energies, and well below those of electronic transitions and dissociations. The difference between 42 and 51 kcalories, however, is not great and could be within the range of experimental error. It is thus not impossible that the energy measured (of the order of 2 eV) is characteristic of the detector and not of the gas. Finally, it must be said that the measurements on carbon dioxide were made before the measurements on nitrogen and that the better quality of these last measurements reflects nothing more than an improvement in experimental technique.

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BIOLOGICAL SCIENCES

How Mosquito Pupae escape from the Surface

MANY insects are known to make use of surface energy effects to allow them to respire at the surface of the water. The outer walls of the respiratory trumpets of mosquito pupae are hydrophil while the inner lining is hydrophuge¹. When the pupae are at the surface the surface film is distorted upwards around the spiracles and the pupae are in a stable position². The positive buoyancy of the pupae is counteracted by the attraction between the water molecules and the outer walls of the spiracles.

Before the pupae can escape from the surface this attraction must be broken and, as with other insects, some special mechanism is needed. Movements of the abdomen and the cephalothorax provide the propulsive force for swimming³, but these alone do not always allow the pupae to escape from the surface. I have found in the pupae of *Aedes togoi* and *A. aegypti* a mechanism at the base of the prothoracic spiracle which produces, under muscular control, a backward movement of the spiracles and which aids in the escape from the surface. It has already been suggested that the spiracles can be moved² but the mechanism involved has not been described nor the proposed function suggested.

Cine film was taken, at a speed of sixty-four frames per second, of pupae escaping from the surface. First, the abdomen was seen to extend and at the same time the spiracles were pulled backwards (Fig. 1). The pupae then made a rapid backward movement of the cephalothorax and moved downwards away from the surface as described before³. Contact between the water surface and the spiracles is therefore broken before the main propulsive movement begins. When the pupae are swimming beneath the surface of the water the spiracles are always pulled backwards as the abdomen is extended. That this is an active movement of the spiracles was confirmed by holding the pupae, attached to fine glass rods, beneath the surface and observing the swimming movements of the static pupae.

The respiratory trumpets can be moved backwards as soon

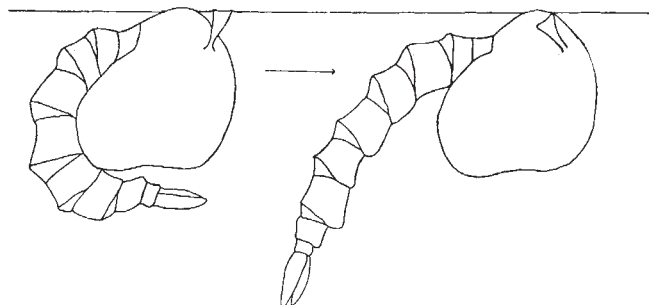


Fig. 1 The initial movements in the escape from the surface of a pharate adult of *Aedes aegypti*. On the left the animal is in its normal resting position at the surface. As the pharate adult escapes from the surface the abdomen is extended and the prothoracic spiracles are pulled backwards so breaking their contact with the surface.

as the pupa emerges from the larval cuticle and the developing adult inside the pupal cuticle (the pharate adult) can also move the spiracles. At the base of each pupal prothoracic spiracles is a rod of pupal cuticle which runs anteriorly and inwards and is attached through the pharate adult cuticle to a pharate adult muscle. The structures can easily be seen through the pupal cuticle. Contraction of the muscle pulls the bar forward, invaginating a small region on the posterior surface of the spiracle and therefore pulling the spiracle backwards. The horns return to their upright position by the elasticity of the pupal cuticle.

To determine whether the backward movement of the spiracles is necessary for the escape from the surface the ability of the pharate adult to move the spiracles was destroyed. A fine wire was inserted through the pupal cuticle and the connexion between the pupal and adult cuticles was broken. The animals were anaesthetized by a short exposure to low temperatures before the operation. The pharate adults were given a few hours to recover and only active animals that could not move their spiracles were used. However, the spiracles were still flexible at their base. Sham operations were carried out in which holes were made in the pupal cuticle above and behind the spiracles. These animals were allowed to recover and could still control the position of their spiracles. Adults later emerged from both operated and sham operated pharate adults. About twenty animals were observed in each category. (The animals were observed at 25° C.)

Animals that could not move their spiracles backwards did not escape from the surface as often as the sham operated or the normal insects (Table 1). Fifty rapid abdominal movements, usually elicited by light mechanical stimulation, were watched for each animal and the number of times that these resulted in the escape from the surface is expressed as a percentage of the total number of attempts. When contact between the water surface and the spiracles was not broken during the initial movements of the abdomen and cephalothorax the propulsive force pushed the animal forward for about a centimetre while they remained attached to the surface.

Usually the escape from the surface of the pupae and pharate adults is extremely rapid and occurs in response to several stimuli¹. The ability to break away from the surface and to dive rapidly must be very important when pupae are escaping from surface predators and then the advantage of movable spiracles becomes apparent.

Table 1 Successful Escapes of Pharate Adults of *Aedes aegypti* from the Surface

Pharate adults unable to move spiracles	Sham operated pharate adults unable to move spiracles	Normal pharate adults
53.4 ± 7%	89.9 ± 1.8%	96.9 ± 0.3%

Successful escapes are expressed as a percentage of the total number of attempts (mean ± s.e.).

The previous suggestion that the movements of the spiracles closed off at the base the spiracle from the rest of the tracheal system during swimming and so avoided the loss of gas² was not confirmed. When pupae that could not move their spiracles were swimming under water no gas was seen to escape from the spiracles.

Anopheline pupae also seem to be able to move their spiracles⁴ and there are structures at the bases of their spiracles which are similar to those found in culicine pupae. Some culicine pupae insert their prothoracic spiracles into the roots of aquatic plants for respiration. These pupae have a similar type of arrangement at the base of their prothoracic spiracles as that found in air-breathing pupae⁵. In this case, however, the movements of the spiracles may be related to their insertion into the plants.

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"New" Sense Organ of Primitive Gastropods

AN array of distance chemoreceptor organs sensitive to the scent of predatory starfish has been found on the ctenidial leaflets of the marine snail *Tegula funebris* Adams. Each leaflet has one such organ; a slight swelling of the efferent border of the leaflet near the base marks its position. The swelling is cut across by a notch whose dorsal wall is infolded to form a pocket lying within the swelling. The swelling with its notch and pocket is termed a bursicle (small pouch) (Figs. 1 and 2).

The bursicles are positioned directly in the path of the main respiratory current flowing between the ctenidial leaflets; tracts of sampling cilia on both lateral faces of each leaflet direct a portion of this current into the bursicle of that leaflet (Fig. 2). Microscopic examination of living leaflets immersed in a suspension of carbon particles in seawater shows that each bursicle possesses a filtering system which keeps the

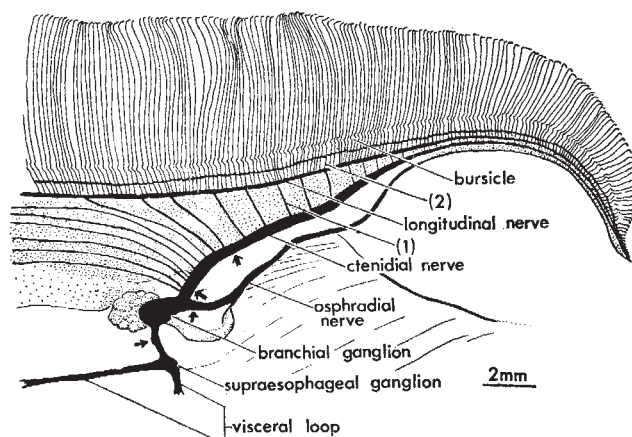


Fig. 1 Anterior half of the ctenidium of *Tegula funebris* showing the major nerves of the ctenidium. 1, Nerve fibres from the ctenidial nerve to the longitudinal nerve; 2, nerve fibres from the longitudinal nerve to the leaflets. Arrows indicate where nerve cuts were made.

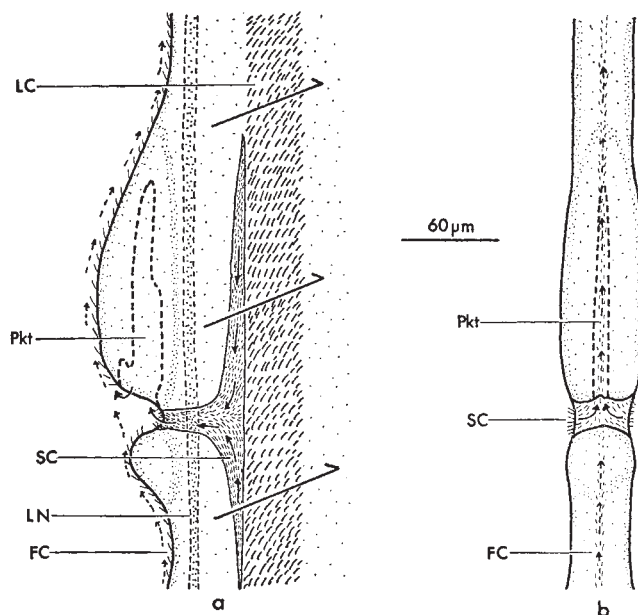


Fig. 2 A bursicle. *a*, Viewed from the side of the leaflet; *b*, viewed "edge on". FC, Frontal cilia; LC, lateral cilia; LN, leaflet nerve; Pkt, pocket of the bursicle; SC, sampling cilia. The single bladed arrows indicate the direction of the respiratory water current, the solid headed arrows indicate the direction of water flow along the tract of sampling cilia, and the broken arrows indicate the current created by the frontal cilia.

interior of the pocket free of debris. Particles carried into the pocket proceed only a short distance before being caught up by cilia, compacted into a bolus and then ejected to be carried away by the frontal cilia. Within the pocket two types of cilia can be distinguished by their activity. Those on the side nearest the efferent blood sinus of the leaflet beat rapidly with no metachronal pattern visible. The cilia on the cells lining the rest of the pocket beat more slowly and have a distinct metachronal rhythm; C-shaped waves originate at the blind end of the pocket and move towards the mouth. The arms of the Cs face the mouth and the waves grow larger and coalesce as they move.

The interior of the pocket is lined with three cell types (Fig. 3). A double row of strongly ciliated cells (*sc*) lies against the supporting rod (*sr*) lining the efferent blood sinus. Each of these cells bears a crown of cilia and has a dark band of basal bodies and ciliary rootlets traversing the cell. The nucleus lies off to the side. It is the cilia of these cells which have a rapid beat. To either side of this double column lies a row of disk-shaped cells (*ds*) with large nuclei; these cells are weakly ciliated. The remainder of the pocket is lined with smaller weakly ciliated cells (*cs*). The cilia of these last two cell types beat metachronally. A layer of nerve fibres (*n*) and nerve cell bodies (*nc*) surrounds the pocket and the strongly ciliated cells send processes into it, suggesting that they are the receptor cells. As the pocket narrows towards its blind end, the nerve

layer becomes thicker and the nerve cell bodies more numerous, and nerve fibre tracts cross the supporting rod to enter the leaflet nerve (*ln*).

The experimental evidence for the bursicles functioning as distance chemoreceptors came from a study of the escape response of *Tegula funebris* to the starfish *Pisaster ochraceus* Brandt. When water carrying the scent of the predator *Pisaster* is allowed to flow into an aquarium containing *Tegula funebris*, the snails respond by rapidly crawling out of the water¹⁻⁵. This response can also be elicited by contact with the starfish. A group of snails which responded consistently to starfish scent by leaving the water was anaesthetized by immersion in an isotonic solution of magnesium chloride and various nerves within the ctenidium were severed. Ninety-four per cent of the snails survived and continued to respond strongly to contact with *Pisaster*. Fig. 1 shows the innervation of the ctenidium and the points where nerve cuts were made. Table 1 gives the results of the post operational scent test; those snails with either the entire ctenidial innervation severed or those with the innervation to the ctenidial leaflets cut failed to respond to *Pisaster* scent: the others responded normally. In a second series of operations, the bursicles were destroyed by cauterization in one group and the behaviour of these snails was compared with that of a control group in which the efferent margin dorsal to the bursicles was cauterized. Survival after these operations was considerably lower than in the first set of operations (Table 2). To expose the ctenidium for cauterization, the portion of the shell over the mantle cavity was removed and the roof of the mantle slit open. Of those

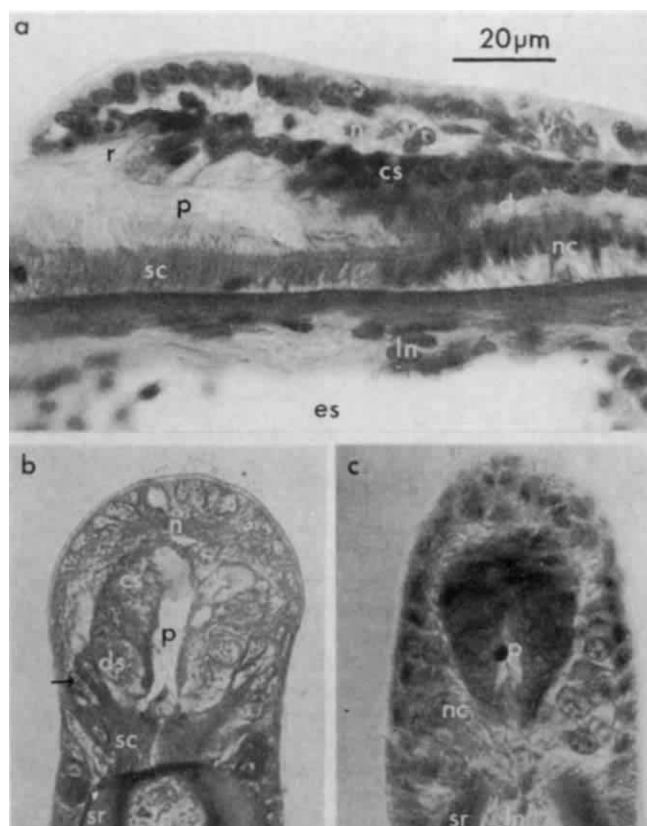


Fig. 3 Sections through a bursicle fixed in Bouin's and stained with Masson's stain. *a*, A longitudinal section passing through the midline of the pocket at its mouth but angled slightly so as to pass through the cells lining the sides of the pocket at the blind end. *b*, A cross-section through a bursicle about a third of the way back from the mouth; the arrow points to the nucleus of a "strongly ciliated cell". *c*, A cross-section through a bursicle near the blind end of the pocket showing the nerve fibres crossing the supporting rod. *cs*, Weakly ciliated cells; *ds*, disk-shaped cells; *es*, efferent sinus of the leaflet; *ln*, leaflet nerve; *n*, nerve layer; *nc*, nerve cell body; *p*, pocket; *r*, recess in the mouth of the pocket where debris is compacted before ejection; *sc*, strongly ciliated cell; *sr*, supporting rod.

Table 1 Responses of *Tegula* to the Scent of *Pisaster* after Ctenidial Nerves have been Cut

Treatment group	Number crawling out/number in group	
	Tested 24 h after surgery	Tested 48 h after surgery
Connective between branchial and supraoesophageal ganglia cut	3/19	4/19
Osphradial nerve cut	18/18	16/18
Ctenidial nerve cut at its origin	5/14	1/14
Ctenidial nerve cut distally	17/17	15/17
Control, narcotic only	19/20	17/20

Table 2 Responses of *Tegula* to the Scent of *Pisaster* after Cauterization of the Ctenidium

Treatment group	% surviving surgery and passing "contact test"	Number crawling out/number in group
Cauterization above bursicles	59	17/20
Bursicles cauterized	52	4/22

animals surviving this treatment, the snails with intact bursicles responded to the scent of *Pisaster*, the snails with bursicles cauterized did not; all animals gave a strong response to contact with *Pisaster* (Table 2).

I surveyed a number of marine gastropod species in California and found bursicles in all species of Zygobranchia (*Fissurella*, *Megathura*, *Puncturella*, *Haliotis*) and all species of Trochacea (*Calliostoma*, *Tegula*, *Astrea*, *Homalopoma*) examined. They were not present in representatives of the Patellacea nor in any Meso- or Neogastropods. In the literature there are scattered references to "nicks" or "glandular swellings" on the efferent margin of the ctenidial leaflets of Zygobranchia⁶⁻⁸ and Trochacea⁷⁻¹⁰. From the authors' descriptions, it is apparent that these structures are bursicles.

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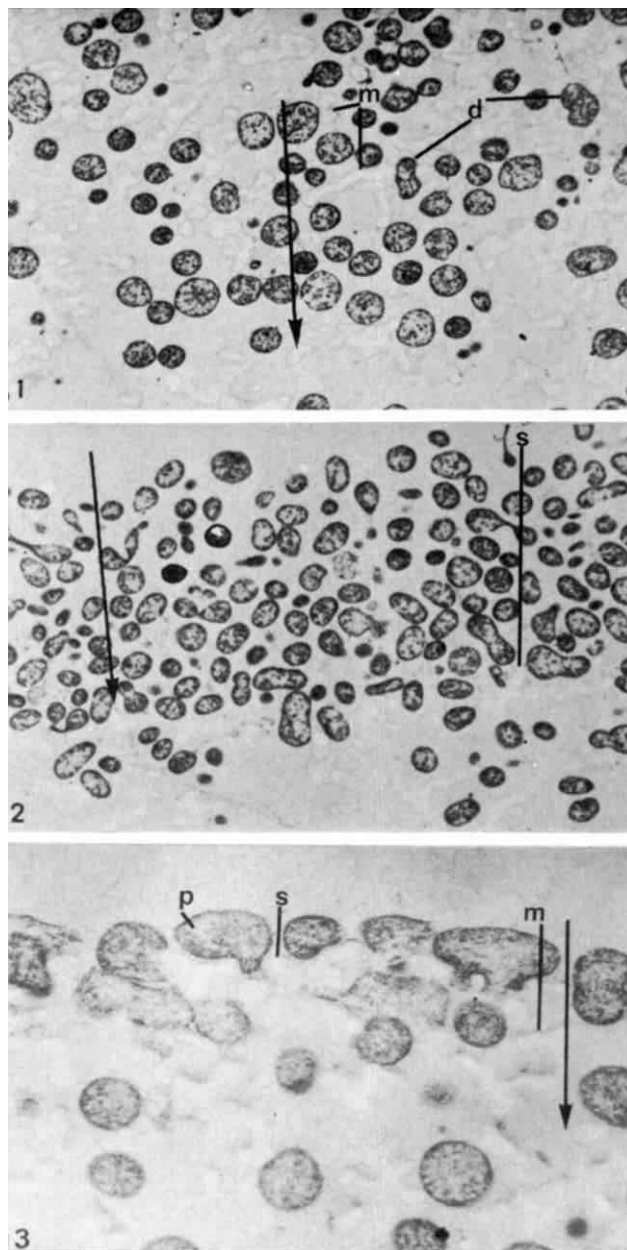
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Figs. 1-3 Cross-sections of membrane filters examined by electron microscopy. After filtration of unfixed or prefixed cultures, membranes were fixed by filtering 2.5% (v/v) glutaraldehyde in 0.01 M sodium cacodylate, pH 7.2, for 5 min and then by immersion in the same fixative for 1 h. Secondary fixation in 1% (w/v) OsO₄ for 1 h, followed by 0.5% (w/v) uranyl acetate for 2 h, dehydration and embedding in araldite. Sections were stained with Reynolds lead citrate¹³. The figures correspond to lines 1, 2 and 6 of the table. s, m, Surface and matrix of membrane; d, cells deformed by matrix; p, cell with "pseudo-podium" in pore. The arrows indicate the direction of filtration. Figs. 1 and 2, $\times 5,700$; Fig. 3, $\times 15,720$.

Sizing Small Organisms

THE smallest spherical cell capable of an independent existence is believed to be, in theory, 100 nm. Bodies of this size have been found by electron microscopy in cultures of mycoplasmas but they are probably inviable^{1,3,4}. The existence of living mycoplasmas as small as 125 nm is indicated by their ability to pass through filters of known porosity⁵⁻⁸, but the following experiments with *Mycoplasma laidlawii* lead one to believe that they are similar to erythrocytes in that they have an elastic cell wall and can therefore be squeezed through membrane filters with pores much smaller than their own diameters. They may, therefore, be larger than the smallest values quoted for mycoplasmas in the literature.

Cultures of *Mycoplasma laidlawii*, type A, ATCC 14089, were grown for 16 h in a prefiltered liquid medium until they contained $3.5-15 \times 10^7$ colony forming units/ml. They were then filtered at room temperature through membrane filters (type MF, average pore diameter (a.p.d.) 450 or 220 nm, 'HAWP' or 'GSWP', Millipore) sterilized by ultraviolet irradiation. The various conditions are specified in the table. With unfixed cultures, organisms were found by electron microscopy within the pores of the membranes, although they did not penetrate to the bottom of 220 nm membranes if the filtration time was restricted (Fig. 1; Table 1, lines 1, 3 and 4).

In contrast, all organisms which were prefixed by the addition of 1 ml. of 2.5% glutaraldehyde in 0.01 M sodium cacodylate (pH 7.2) to 9 ml. of culture were retained at the surface of 220 nm membranes and the majority at the surface of those of a.p.d. 450 nm (Fig. 2; Table 1, lines 2 and 5). There was no evidence to suggest that prefixed organisms were excluded by either shrinkage of the membrane filter or precipitation of the medium. The fixative presumably increased the rigidity of the normally soft cells so that they could no longer be squeezed into the pores. Unfixed cells were apparently more easily deformed and some distorted cells were visible even within the pores of 450 nm membranes (Fig. 1).

With a vacuum of 10 mm Hg and a filtration time of 15 min, some organisms entered the filtrate when the a.p.d. was 450 nm

Table 1 Filtration of *Mycoplasma laidlawii*

Vacuum (V) or pressure (P) (mm Hg)	Porosity of membrane filter (nm)	Average flow rate (ml./min)	Time of filtration (min)	% CFU in filtrate	Fixation before filtration *	Organisms throughout membrane	Organisms deformed
10 (V)	450 †	0.8	15	6.6	—	+	+
10 (V)	450 †	0.3	15	n.t.	+	—	—
10 (V)	220 †	0.6	15	<0.0001	—	—	++
10 (V)	220 †	0.5	30	0.002	—	+	++
10 (V)	220 †	0.3	15	n.t.	+	—	—
>80 (P) §	220 †	2.0	4	0.015	—	+	+++
80 (P)	220 †	0.4	5	n.t.	—	+	++
40 (P)	220 †	0.3	5	n.t.	—	+	++
20 (P)	220 †	0.1	15	n.t.	—	—	++

* 9 ml. culture fixed with 1 ml. 2.5% (v/v) glutaraldehyde in 0.01 M sodium cacodylate, pH 7.2, for 15 min at room temperature.

† Membranes of 25 mm diameter in Millipore holder (No. XX10 025 00).

‡ Membranes of 13 mm diameter in Swinny hypodermic attachment (No. XX30 012 00).

§ Swinny holder attached to hand-operated hypodermic syringe; exact pressure not determined, but comparison with flow rates under controlled pressures suggests that it exceeded 80 mm Hg.

|| Cells sparse at bottom of membrane.

CFU, Colony-forming units. n.t., Not tested.

but not when it was 220 nm (Table 1, lines 1 and 3). Other cultures were filtered successively through membranes of a.p.d. 800, 650, 450 and 220 nm to remove large cells that might have blocked the finer pores and retained any unusually small cells; but again, no cells (<0.0001%) passed the 220 nm membrane during filtration for 15 min under a vacuum of 10 mm Hg, and electron microscopy of sections did not reveal any cells of less than 300 nm diameter within the 220 nm membrane. Nevertheless, viable organisms passed through a 220 nm membrane when the filtration time was doubled (Table 1, line 4) or when it was mounted in a Swinny holder on a hand-operated syringe (Table 1, line 6). If the organisms passing a 220 nm membrane after prolonged filtration were refiltered through a similar membrane under the same conditions, 99.9% were retained. Thus, cells larger than the a.p.d. had been forced through the first membrane. With the Swinny holder, the flow rates obtained with known pressures of compressed air showed that hand operation produced unexpectedly high pressures, with consequent distortion and even disruption of the organisms (Fig. 3; Table 1, line 6).

These observations show how easily unfixed mycoplasmas can be made to pass membranes of small a.p.d. unless gentle conditions are used. This probably depends more on the plasticity of the organisms than on their size, and the results are largely determined by the conditions of filtration. Whenever mycoplasmas have been reported as passing filters of a.p.d. less than 390 nm, severe conditions were used which may have forced organisms through the pores, for example, 100–760 mm Hg^{5,7} or hand-operated Swinny filters^{8,10,11}, in which considerable pressures can evidently be produced.

Although the genome size of *Mycoplasma laidlawii*¹² suggests that this organism is one of the larger mycoplasmas, all members of the Mollicutes lack a rigid cell wall and probably behave similarly during filtration. The general conclusion is, therefore, that the smallest viable mycoplasmas may be larger than some filtration experiments suggest.

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Movement as a Necessary Stimulus of *Octopus* Predation

It is usually supposed that movement is a necessary stimulus to initiate the attack of *Octopus vulgaris* on a crab and, for example, Boycott and Young say¹ that "this movement is absolutely necessary; a crab which remains still, even in full view of an octopus, is not attacked". In many experiments, the presentation of stimuli has been based on this conclusion; the stimuli were moved to attract the octopus^{2,3}. My observations indicate, however, that *O. vulgaris* frequently attacks crabs which are not moving at the time and for some time before.

There is some earlier evidence to suggest as much. First, Wells noted that *O. vulgaris* will manipulate anything, even inanimate objects, placed into its aquarium². Second, the hunting behaviour of *O. cyanea* includes pouncing on coral heads, rocks, clumps of algae and other objects⁴. Yarnall interprets these pounces as "speculative", the octopus mistaking objects that look like crabs, but I consider that the objects are probably mistaken for molluscs. Third, it is known that octopuses feed on lamellibranchs and gastropods^{5–8} which shows that, if movement is necessary, it need be no more rapid than a "snail's pace" or the opening and closing of a bivalve. Fourth, if the aperture of an empty gastropod shell is completely sealed, *O. vulgaris* responds as though it contained a live snail⁸, and, when given a choice between an empty sealed shell and a live snail, will not discriminate between them⁸. Fifth, I have seen *O. vulgaris* leave the water, climb on exposed rocks, pull various immobile gastropods (such as *Nerita* spp.) off the rocks, return to the water and feed on them. Sixth, many species of octopus and their crustacean and molluscan prey

are nocturnal, so that senses other than vision are implicated. Seventh, blinded *O. vulgaris* will feed when chemotactically stimulated². Finally, my observations suggest that *O. vulgaris* is also a scavenger, feeding on dead fishes, crustaceans and molluscs.

These observations indicate that movement of prey is not a necessary stimulus for predation. The present experiments, in which all appendages except the maxillary palps were removed, prevented locomotion in *Callinectes ornatus*, *C. marginatus*, and *C. exasperatus* before presenting them to the octopus. Eighteen different specimens of *O. vulgaris* Lamarck (mean weight = 555 g) were individually maintained in opaque fibreglass aquaria (55.9 × 41.3 × 25.4 cm) supplied with running seawater. In the first experiment, six crabs were presented during daylight hours and six at 2230 h on a full Moon night. In the daylight experiment, five crabs were immediately attacked, but the sixth octopus did not touch its crab during four subsequent hours. By the next morning, however, the crab was eaten. Five of the six crabs presented at night were covered by the interbranchial webbing of the octopuses when inspected an hour later, but it is not known how soon they were attacked. At the time, the sixth crab was untouched, but all were eaten by morning.

Although ten of the feedings might be interpreted as stimulated by movement when placing the crab in the aquarium, the two cases in which the crab was untouched for 1–4 h cannot be so interpreted. In at least these cases, movement from neither the experimenter nor the crab could have been the initiating stimulus for predation.

In the second experiment, six octopuses had eaten conch *Strombus raninus* less than 12 h earlier so as to decrease the motivation to attack and feed. Two legless crabs were placed simultaneously in the two corners of the aquarium away from the octopus. Observations were made at 5 min intervals for the first hour, at 15 min intervals for the second hour and five additional checks were made during the next 6 h. No crab moved from its original position. When the crab was under the interbranchial webbing, a time score was assigned which was the midpoint between the two closest observations.

The first crab was taken in a mean time of 43.3 min (median time = 32.5; range from 2.5 to 87.5). Only one crab was attacked in less than 10 min. The second crab was taken in a mean time of 211.7 min (median = 210.0; range from 27.5 to 460.0). Four octopuses ate the first crab before taking the second, whereas the other two gathered both crabs at the same time.

Latencies of attack of either the first or second crab are simply too long to be attributed to any initial disturbance or movement by the experimenter. It is obvious that motion is not an "absolutely necessary" stimulus for attack, but the experiments do not indicate the stimuli to which these experienced octopuses were responding.

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Dissociation of the Circadian Drinking Pattern from Eating

RATS allowed food and water *ad lib.* show a circadian rhythm of drinking and a close association of water intake with eating. One estimate of the proportion of total water taken at night is 78%¹. Meals apparently influence the timing and the amount of water drunk. About 57% of the total drinking occurs within 20 min of meals². A meal introduces solutes into the blood and shifts water from the body fluids into the gut. To compensate for this, between 1 and 1.5 ml. of water would need to be drunk for each gram of food eaten³. Water requirements over a longer term are also determined by the diet⁴, because of obligatory renal water losses occasioned by solutes from the food.

After water deprivation too, drinking seems to depend on eating. Rats deprived of water at various times of day drink different amounts as a function of the times of day during which deprivation occurred⁵. It is not the time of drinking as such that determines this variation, but circadian differences in the amounts of food eaten during the deprivation intervals. Rats injected with salt at different times show no circadian variation in drinking⁶.

These data suggest that this time-dependent variation of water intake depends solely on the circadian rhythm of eating. But Fitzsimons⁷ found that rats continuously infused with liquid food continued to drink, mostly at night, and Fitzsimons and Le Magnen⁸ found that rats fed twice a day when lights were on took 50% of their total water at night. Rats deprived of food also continued to drink mostly at night. A diurnal influence not determined by eating was affecting drinking.

In the experiments to be described here male hooded rats weighing 281 to 374 g each lived in a box where eating and drinking were continuously recorded. They were fed first *ad lib.*, and then at a constant rate by delivering small meals (approximately 0.66 g) every hour, or normal sized meals (approximately 1.66 g) every 2.4 h.

Each rat lived for 2–5 weeks in the experimental box (24 × 22 × 18 cm) within a sound muffled chamber. Light from a 3 W bulb in the box was switched on at 0800 h and off at 2000 h. Temperature was maintained at between 21° and 26° C. Water was always available from a drinking tube, tongue contacts with which were detected by a drinkometer, and the number of seconds spent drinking were recorded. Opposite the drinking spout was a magazine into which 54 mg food pellets (diet 41 b) were delivered. Data were recorded on a cumulative recorder.

During 3 days' habituation to the apparatus and for the first part of the recording period, food was available *ad lib.* Earlier rats in the series delivered a further pellet by withdrawing their head from the food magazine, thus completing an infrared photo-beam. Later the photo-beam was used to detect the pellet in the magazine⁹. This made no discernible difference to the rats' behaviour. In the analysis the criterion for a meal was eating at least three pellets, without pauses of more than 10 min. A drink was defined as licking for at least 2 s, with pauses of no more than 2 min.

After eating *ad lib.*, each rat was deprived of food overnight and then maintained on the periodic schedule of meals without being able to influence food delivery. For three rats scheduled means were of twelve pellets each, at 1 h intervals, and for three they were of thirty pellets at 2.4 h intervals. One rat after the *ad lib.* period had the 1 h and then the 2.4 h schedule. Within a meal one pellet was delivered every 7.5 s. During food deprivation rats lost a mean of 16.5 g body weight, and under the scheduled meal regimes they maintained weight or grew slowly. At the start of scheduled meals not all the rats ate meals promptly, but by the fifth meal all were taking the first pellet before the second was delivered, and continued to eat every meal completely and immediately.

Fig. 1 shows the intake of one rat for 27 days after habituation. *Ad lib.* eating had a characteristic pattern. The fine

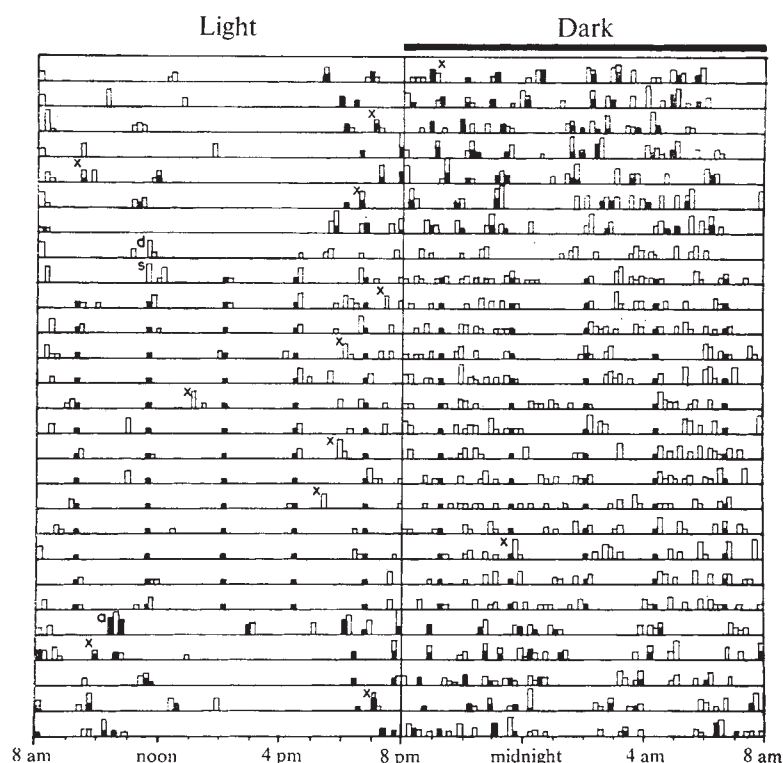


Fig. 1 Histogram showing the eating (filled bars) and drinking (open bars) of a single rat for 27 days after the usual period of habituation to the box. Each row represents a single day starting at 0800 h. A full scale open bar represents a single drink of 50 s and a full scale filled bar a meal of 20 min. Pauses within the bout of intake were excluded from these times and the meal or drink was localized on the graph within the nearest 10 min. At the points marked *x* the rat was removed for about 10 min and weighed. Cleaning and refilling also took place at these times. Return of the rat to the box was almost invariably followed by a drink, or less commonly a meal, and for this reason the box was opened at different times of day. At the other points, marked by letters, the rat was also removed and replaced. In addition, at *d* it was deprived of food. At *s* the scheduled meal regime started, and at *a* the rat was again given food *ad lib.*

detail of such patterns seems to be pre-programmed¹⁰, and is idiosyncratic. During the initial *ad lib.* period the rat of Fig. 1 consumed little during the day, until about 1800 h, when the onset of active intake anticipated the onset of dark by about 2 h. An important role of biological rhythms is to predict and perhaps slightly anticipate periodic changes in the environment. At night the rat ate and drank steadily except for a pause of 2 h starting at about midnight, and another starting at about 0600 h. During deprivation of food the characteristic pattern of drinking was virtually unchanged. This suggests that in rats deprived of food, drinking, which seems to be in excess of their water requirements^{11,12}, is a consequence of the continuing activity of a mechanism generating drinking in a pre-programmed sequence. The pattern continued without substantial change during scheduled meals, and was unaltered except for a few hours after the return to *ad lib.* feeding.

The rat of Fig. 1 was typical of all animals given scheduled meals, and Table 1 shows their mean data. Delivery of food at 1 h or 2.4 h intervals resulted in only a small drop of total drinking. The mean proportion of water consumed during darkness decreased by 2% with the change to scheduled meals. On every day of *ad lib.* recording every rat drank more in the

dark than in the light ($P < 0.001$ on a signs test) and every rat (except one on a single day) drank more in the dark on every day of scheduled meals ($P < 0.001$). There was more *ad lib.* eating during the dark on all but 2 recorded days ($P < 0.001$), whereas during scheduled meals, eating was of course equal during light and dark. *Ad lib.* drinking is thus relatively independent both of growing water deficits, which often exceed theoretical threshold during normal drinking in the day, and of the influence of eating.

Dissociation of drinking from eating is even more complete than is implied by this finding that the circadian rhythm of drinking continues during a constant rate of eating. Drinking immediately before and after a meal, during *ad lib.* feeding, had a higher probability than during the rest of the inter-meal interval (Fig. 2). Under the scheduled meal regime drinking associated with meals decreased, and drinking between meals increased. Ratios of the mean proportion drunk within 8 min after a meal to the mean drinking per 8 min more than 16 min from a meal were calculated for both day and night, and these ratios for each rat, feeding *ad lib.*, were compared with those for scheduled meals. Ratios of drinking before meals to inter-meal drinking were compared in the same way. In every com-

Table 1 Mean Data of Rats eating first *Ad lib.* and then Scheduled Meals

	No. of rats	Total days	Mean time spent eating (excluding pauses) per 12 h		Mean time spent drinking (excluding pauses) per 12 h		Mean ratios of drinking in 8 min before or after a meal to drinking in 8 min more than 16 min from a meal			
			(min)		(s)		(< 8 min before: > 16 min)		(< 8 min after: > 16 min)	
			Light	Dark	Light	Dark	Light	Dark	Light	Dark
<i>Ad lib.</i>		18	28.1	51.1	173	521	8.0	4.0	38.9	20.1
1 h scheduled meals	4	21	18	18	173	424	0.9	1.0	3.1	2.1
<i>Ad lib.</i>		28	16.8	47.5	162	527	5.2	3.4	21.1	11.7
2.4 h scheduled meals	4	47	19	19	156	477	1.1	1.5	3.9	2.9

The *ad lib.* scores for one rat which had the 2.4 h and then the 1 h scheduled meals are included in both sections of the table, otherwise the data in each section are for different animals. Days on which an apparatus failure occurred are not included in the analysis, nor are any days immediately after a failure that interrupted intake.

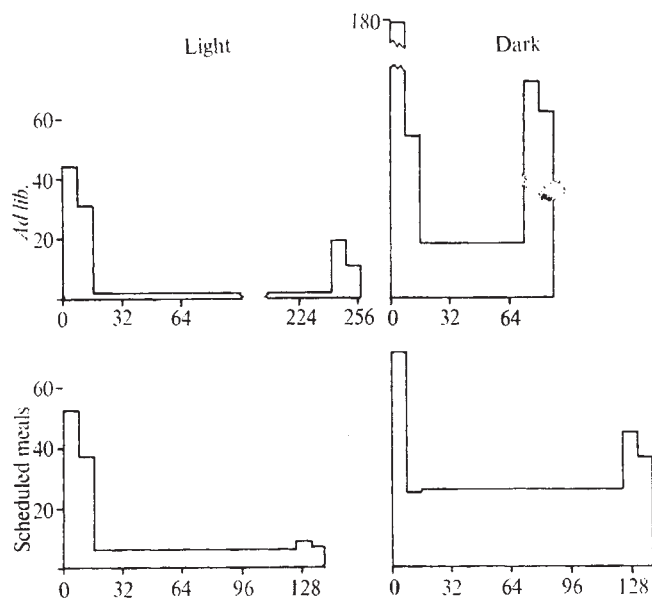


Fig. 2 Histograms showing the mean amount of drinking per 12 h as a function of time since the end of the last meal in the four rats given first *ad lib.* food (top histograms) and then scheduled meals at 2.4 h intervals (bottom histograms). Histograms on the left are for the period of light and on the right for the period of darkness. The ordinate is the total number of seconds spent drinking by each rat per 8 min period since eating per 12 h. The abscissa is the interval since the ending of the last meal in min. The horizontal extent of each histogram is the mean inter-meal interval. Only drinking up to 16 min before or after each meal was calculated separately. Over the rest of the inter-meal interval the drinking was averaged.

parison for every rat the ratio decreased with the shift from *ad lib.* to scheduled meals. Pooling the comparisons for day and night, for the four rats in each condition, the decrease in meal-associated drinking was significant ($P=0.004$ in signs tests with $n=8$) for drinking both before and after meals, and under both 1 and 2.4 h meal schedules. Means of the ratios across the four rats in each schedule condition are shown in Table 1.

There is some increase in the probability of drinking immediately after the delivery of scheduled meals (Fig. 2). The peak of drinking before meals virtually disappears, however, as would be expected unless the animal were making a difficult timing discrimination. To make anticipation easier, for one further rat scheduled meals of thirty pellets were delivered at pseudo-random intervals (mean 2.4 h). Three minutes before each an extra pellet was delivered to signal that a meal was about to arrive. During 13 days of this regime the rat did not learn to drink in anticipation of meals.

These observations all suggest that the hypothesis of Fitzsimons and Le Magnen⁸ that drinking associated with meals is a learned response anticipating the coming water requirements caused by the meal is incomplete. The following hypothesis seems to fit all the observations so far. During *ad lib.* intake, drinking is not regulated primarily by physiological needs or deficits, but is controlled by an independent (presumably neural) programmer which has a marked circadian rhythm, and is responsive in the same way as other diurnal oscillators to such events as reversal of the light-dark cycle⁷. The meal programmer has similar properties, and in *ad lib.* conditions both mechanisms are entrained so as to bring about the close association of eating and drinking. Entrainment between non-linear oscillators is well known¹³, but the interconnexions between eating and drinking that stabilize the phase relationships of the two types of intake need to be investigated. They may include disinhibition of eating by drinking¹⁴, dry mouth factors or fluid losses produced by a meal stimulating drinking³, and even learning⁸. These events would not normally serve to cause eating or drinking directly,

as has been widely assumed. Instead they would serve to synchronize the oscillatory programming mechanisms so as to maintain the association between meals and drinks. The principle whereby co-occurrence of physiological events may arise, not from direct causal connexions, but from entrainment between biological oscillators, is probably of wide significance.

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Abolition of the Response of Brain Stem Neurones to Iontophoretically Applied *d*-Amphetamine by Reserpine

PREVIOUS studies have indicated that the central excitant action of *d*-amphetamine is the result of direct activation of the brain stem reticular formation^{1,2}. It has been suggested that amphetamine acts on the central nervous system by releasing noradrenaline from presynaptic sites³; and in fact noradrenaline release follows intracisternal administration of *d*-amphetamine⁴, and histochemical studies have demonstrated the existence of nerve terminals containing noradrenaline in the brain stem reticular formation⁵. Studies of the effects of iontophoretically applied noradrenaline to single neurones in the cat brain stem have shown that both excitation and inhibition can occur⁶, and that these actions of noradrenaline are closely mimicked by iontophoretically applied *d*-amphetamine⁷. This could result either from release of endogenous noradrenaline or from a direct effect of amphetamine. Pretreatment with reserpine reduces concentrations of brain amines; the actions of iontophoretically applied *L*-noradrenaline (*L*-NA) and *d*-amphetamine to single neurones in the brain stem have therefore been compared in reserpinized and untreated rats.

Experiments were performed on eighteen rats, seven of which were injected intraperitoneally with 5 mg/kg reserpine (Halewood Chemicals Ltd) 24 h before recording. The rats were anaesthetized with halothane ('Fluothane', ICI) and the medial portions of the cerebellum were removed by suction. Five-barrelled glass micropipettes of tip size 6–8 μ m were inserted through the floor of the fourth ventricle to record spontaneous activity of single neurones and to eject drugs. During recording, anaesthesia was maintained with halothane at a concentration of less than 0.5%. The method of recording and counting neuronal spikes has been described before⁸. Compounds were ejected iontophoretically from the micropipettes with currents of 25–50 nA for one or more 5 s periods.

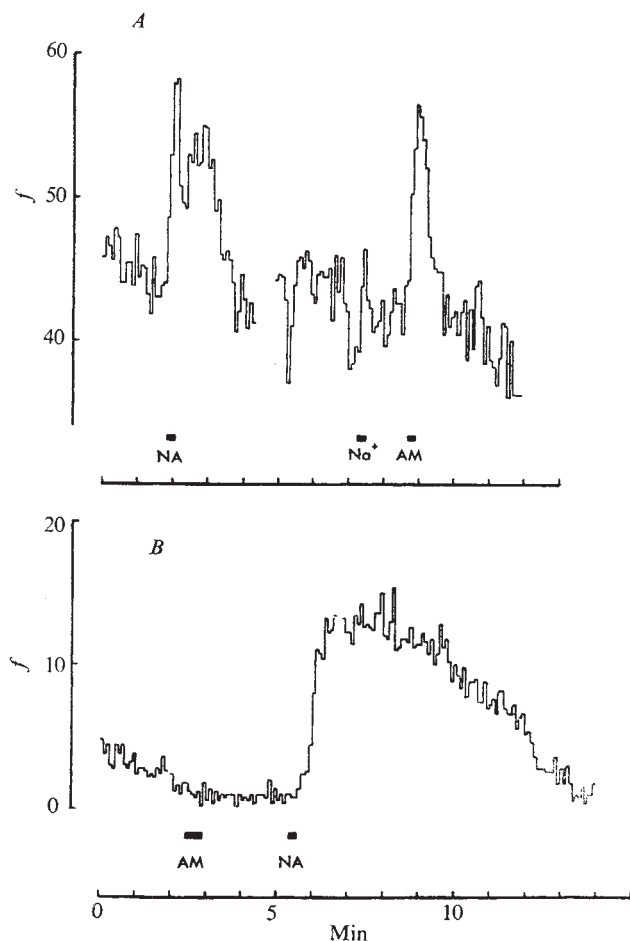


Fig. 1 Effects of *l*-noradrenaline (NA) and *d*-amphetamine on a neurone in an untreated rat (*A*) and on a neurone in a rat pretreated with 5 mg/kg reserpine (*B*). The firing rates of both neurones in impulses s⁻¹ are plotted against time in min. Iontophoretic applications of NA, AM and of a current control (Na⁺) are indicated by horizontal bars. *A*, The excitatory response to NA is mimicked by AM. *B*, *d*-Amphetamine is without effect on this neurone, whereas NA has a strong excitant action.

A retaining current of 15 nA prevented unwanted diffusion from the micropipettes. (The relationship between the iontophoretic current and the amount of compound ejected, and the effectiveness of the retaining current has been investigated by Bradley and Candy⁹.) The following drugs were used in the micropipettes: *l*-noradrenaline base (Koch-Light), 10% at 5.0–6.0; *d*-amphetamine base (Aldrich), 2% at pH 5.0–6.0; *d*-amphetamine sulphate (Menley and James), 2% at pH 5.0–6.0.

A total of seventy-eight brain stem neurones were studied in the untreated rats. The actions of iontophoretically applied *l*-NA on neurones in these animals were similar to the effects of iontophoretically applied *l*-NA on brain stem neurones in the decerebrate unanaesthetized cat^{10–12}. Short lasting and long lasting inhibitory effects, biphasic effects (short lasting inhibition followed by excitation), and excitatory effects were all observed. *d*-Amphetamine closely mimicked the excitatory and inhibitory effects of *l*-NA and had no effect on neurones unaffected by *l*-NA (Table 1*A*). *l*-NA had a biphasic action on twenty-one neurones; *d*-amphetamine had a similar biphasic action on fourteen of these neurones and a short lasting inhibitory action on the remaining seven. An example of excitation by *l*-NA and *d*-amphetamine is shown in Fig. 1*A*.

Fifty-seven brain stem neurones were studied in rats pretreated with reserpine, and the excitatory actions of *l*-DNA were strong and prolonged (Fig. 1*B*); there seemed to be no difference between the short inhibitory actions of *l*-DNA

observed in the untreated animals and those in the reserpinized animals; biphasic responses were rarely seen in the reserpinized animals. *l*-NA excited thirty-four of the neurones studied, but *d*-amphetamine had an excitatory action on only one of these. *l*-NA inhibited thirteen neurones in the pretreated rats and *d*-amphetamine inhibited two of them (Table 1*B*). An example of a neurone excited by *l*-NA but unaffected by *d*-amphetamine is shown in Fig. 1*B*.

Table 1 Comparison of the Effects of Iontophoretically Applied *l*-Noradrenaline (*l*-NA) and *d*-Amphetamine (AM) on the Same Neurones in (*A*) Untreated and (*B*) Reserpinized Rats

(A) Untreated AM					(B) Reserpinized AM				
	+	–	O	(totals)		+	–	O	(totals)
<i>l</i> -NA	+	24	2	2 (28)	<i>l</i> -NA	+	1	0	33 (34)
	–	1	19	1 (21)		–	0	2	11 (13)
	O	0	0	8 (8)		O	0	0	10 (10)
				(57)					(57)

The figures represent the numbers of neurones showing excitation (+), inhibition (–) or no effect (O). Biphasic responses are not included in this table (see text).

The similarity of the neuronal responses to *l*-NA seen in the halothane-anaesthetized rats in these experiments, and those obtained in decerebrate unanaesthetized cats^{10–12}, is interesting because it indicates that halothane anaesthesia does not depress neuronal responses to *l*-NA in the brain stem. This is in agreement with the results of Johnson *et al.*¹³, who found that the responses of cortical neurones to *l*-NA were not depressed by low concentrations of halothane. The increase in the magnitude and duration of the excitatory response to *l*-NA seen in the reserpinized animal is similar to the supersensitivity of peripheral adrenergic receptors observed in reserpinized cats¹⁴. There did not seem to be any increase in the magnitude or duration of the short inhibitory response to *l*-NA in the reserpinized animals, and no prolonged inhibitory effects were observed.

The ability of *d*-amphetamine to mimic responses to *l*-NA in the untreated rats is similar to its action in the decerebrate cat⁷. The noradrenaline content of whole brain is reduced to a very low level after 5 mg/kg reserpine has been given¹⁵, and histochemical studies have shown an almost total disappearance of fluorescence from catecholamine-containing nerve terminals in the brain stem after this dose of reserpine⁵. The almost total abolition of the actions of *d*-amphetamine on brain stem neurones after reserpine pretreatment thus supports the hypothesis that *d*-amphetamine releases endogenous noradrenaline from presynaptic structures.

The possibility remains that reserpine might directly antagonize post-synaptic actions of *d*-amphetamine but not those of *l*-NA. It is hoped that studies of the effects of inhibitors of the synthesis of noradrenaline on the actions of *d*-amphetamine will clarify this problem.

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Difference in Haemodynamic Response to Prostaglandins A and E

PROSTAGLANDINS A and E are vasodepressor substances, the effect of which has been attributed to direct vasodilator action on peripheral arteriolar smooth muscle¹⁻⁷. The mechanism of the hypotensive effect of the two prostaglandins may not, however, be identical. Prostaglandins A differ from prostaglandins E in that the former escape degradation by the lung⁸, and do not contract non-vascular smooth muscle¹. We have evidence that prostaglandin A compounds differ from prostaglandin E series also in the mechanism of their hypotensive action.

Cats (2 and 3.5 kg) were anaesthetized with pentobarbital (1.7–2.8 mg/ml., 0.9% saline) at a rate of 5 mg/kg/h into the right femoral vein. For prostaglandin injection, a polyethylene catheter was introduced into the thoracic aorta through the left femoral artery. Right femoral arterial blood pressure was monitored continuously by means of a Statham strain gauge (P-23DC) and recorded on a Grass polygraph (model 7). The animals were heparinized (sodium heparin 200 U/kg). Prostaglandin A₁ (PGA₁), prostaglandin A₂ (PGA₂), prostaglandin E₁ (PGE₁) and prostaglandin E₂ (PGE₂) (5–120 ng/2 ml., 0.9% saline) were injected into the thoracic aorta before and after the administration of pentolinium (10 mg given intraperitoneally). The diastolic decrease in blood pressure was analysed with respect to the time after injection and the data were subjected to Student's *t* test. In the absence of pentolinium, the diastolic blood pressure was 105–140 mm Hg, whereas after pentolinium, the diastolic blood pressures were 75 and 100 mm Hg. There was less variation in baseline blood

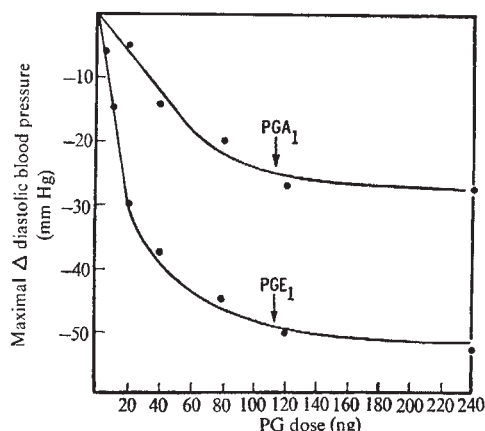


Fig. 1 Relationship between prostaglandin dose and maximal fall in blood pressure. Representative experiment, pentobarbitalized pentolinium-treated cat. Prostaglandins injected into the thoracic aorta.

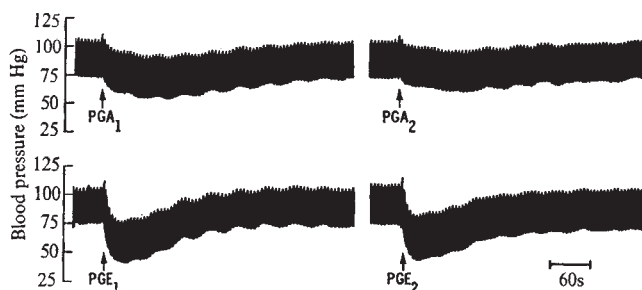


Fig. 2 Recording of pressure following prostaglandin administration. Representative experiment in a single animal. Prostaglandin dose: 80 ng intra-aorta; otherwise, the same conditions as Fig. 1.

pressure with ganglionic blockade associated with a reduction in spontaneous blood pressure fluctuations.

Fig. 1 illustrates a typical dose response after injection of PGA₁ and PGE₁ in the pentolinium-treated cat. In these experiments, the maximal decrease in diastolic blood pressure was analysed without regard to the time at which it occurred. With both PGE₁ and PGA₁ there was a curvilinear relationship between the dose of prostaglandin (5–120 ng) and the maximal decrease in blood pressure, but the effect of PGE₁ was greater than that of PGA₁. PGE₂ and PGA₂ had effects similar to those of PGE₁ and PGA₁ respectively. In animals not treated with pentolinium, the response to the prostaglandins was similar but more variable. In all subsequent studies the dose of prostaglandin used was 80 ng.

Representative blood pressure responses to the four prostaglandins in one pentolinium-treated animal are illustrated in Fig. 2. After injection of both PGA₁ and PGA₂ there was a gradual decrease in blood pressure with a relatively slow return to pre-injection levels. In contrast, the decrease in blood pressure after injection of PGE₁ and PGE₂ occurred almost immediately and was greater. With all four prostaglandins, blood pressure gradually returned to control levels by 420 s. To study in more detail the apparent difference in time course between the effects of prostaglandins A and E, data were obtained from six consecutive animals before and after ganglionic blockade.

The time course of the blood pressure response was divided into two phases: the first 80 s after injection and the remaining 340 s. In the non-pentolinium-treated animals (Fig. 3A) PGA₁ caused a gradual decrease in blood pressure during phase 1 (maximum: -10 ± 2 mm Hg at 60 s) with a gradual return to control during phase 2. In contrast, PGE₁ produced an immediate decrease during phase 1 (maximum: -38 ± 5 mm Hg at 20 s). The return pressure was rapid during phase 1, but did not reach control values. During phase 2 there was a gradual return to preinjection values similar to that observed with PGA₁.

The pattern of blood pressure response to prostaglandins A and E was qualitatively identical after ganglionic blockade (Fig. 3B). But ganglionic blockade augmented the blood pressure response to PGA₁ throughout phase 1 and for the early part of phase 2 (maximum: -18 ± 4 mm Hg at 80 s). In contrast, ganglionic blockade did not affect the degree of initial decrease in blood pressure with PGE₁ during the early part of phase 1 (maximum: -33 ± 6 mm Hg at 20 s). The magnitude of decrease in blood pressure with PGE₁ during the remainder of phase 1 and the early part of phase 2, however, was significantly greater in the pentolinium than in the non-pentolinium treated group.

Table 1 shows statistical analysis of the blood pressure response at selected time intervals following administration of PGA₂ and PGE₂ as well as PGA₁ and PGE₁. At 20 s, the hypotensive effect of PGE₁ and PGE₂ compounds was significantly greater than that of PGA₁ and PGE₂ respectively in animals with or without pentolinium. No statistical differences were observed in the degree of decrease in blood pressure

Table 1 Effect of Prostaglandin Injection on Blood Pressure of the Cat

Time (s)	20		80		180	
		<i>P</i>	Non-pentolinium-treated	<i>P</i>		<i>P</i>
PGA ₁	-6.9 ± 1.3		-9.2 ± 1.9		-4.3 ± 0.8	
PGE ₁	-38.0 ± 4.7		-10.2 ± 1.8	NS	-4.3 ± 0.7	NS
PGA ₂	-4.4 ± 1.4	<0.001	-5.0 ± 1.6	NS	-2.8 ± 1.5	NS
PGE ₂	-27.2 ± 4.5	<0.005	-7.1 ± 2.8	NS	-3.8 ± 2.2	NS
			Pentolinium-treated			
PGA ₁	-12.4 ± 2.0		-18.3 ± 4.0	NS	-9.9 ± 1.7	NS
PGE ₁	-32.7 ± 5.5	<0.01	-19.4 ± 5.0	NS	-8.8 ± 3.6	NS
PGA ₂	-9.5 ± 2.9	<0.001	-13.1 ± 3.9	NS	-9.3 ± 3.1	NS
PGE ₂	-29.3 ± 2.6		-16.9 ± 4.0		-6.8 ± 2.3	

Each value represents the mean fall in blood pressure (mm Hg) ± s.e. following intrathoracic aortic prostaglandin injection (80 ng). *n* = 6. *P* = Probability of significance between PGA₁ and PGE₁, or PGA₂ and PGE₂. NS, Non-significant.

among all four prostaglandins from 80 s until the time when blood pressure had returned to control values (phase 2). The pattern and degree of the decline in blood pressure induced by PGA₂ and PGE₂ was almost identical to that of PGA₁ and PGE₁ respectively.

This study shows that the prostaglandins E are more active than A on intra-aortic administration. This does not contradict previous findings suggesting that prostaglandins A are more active than E on intravenous injection^{2,3}, because intravenous injection necessitates passage through the lungs which selectively inactivate prostaglandins E but not prostaglandins A⁸. In any case, direct comparisons between potencies of prostaglandins A and E based on the maximal fall in blood pressure should be made with caution, for the maximal fall in blood pressure occurs at different times with each of the prostaglandin classes which may imply different mechanisms of action.

Prostaglandins A and E have been thought to lower blood pressure by direct arteriolar vasodilation^{1,7,9}. In this study, the early rapid fall following intra-arterial injection of prostaglandins E indeed suggests a direct vasodilator effect on arteriolar vascular beds distal to the point of injection in the thoracic aorta. This early effect was barely observable with prostaglandins A in the non-pentolinium treated animals and absent in the pentolinium-treated group, suggesting that prostaglandins

A do not immediately dilate these vascular beds by direct action. The sustained lowering of blood pressure with intravenous PGA compounds might be explained by the fact that these prostaglandins are not inactivated by the lungs of dogs⁸ and cats³ (and unpublished observations) and presumably continue to circulate with biological activity for some time. In contrast, prostaglandins E, in the dose given, are completely metabolized by the lung in the dog⁸ and in the cat³ (and unpublished observations) and after one passage do not again reach the systemic circulation in active form. The delayed vasodepressor effect following PGE injection thus seems not to be the result of direct peripheral arteriolar vasodilation. Rather, the entire (early and delayed) response to administration of prostaglandin E can probably be attributed to events taking place between the thoracic aorta and the pulmonary circulation during the first few seconds after injection. The following possibilities may be considered as possible explanations for the delayed lowering of blood pressure with prostaglandins E: (1) binding of prostaglandins E by arterioles below the site of injection; (2) dehydration of prostaglandins E to prostaglandins A in organs located below the thoracic aorta (kidneys, gastrointestinal tract, liver, etc.); (3) activation by prostaglandins E of a locally active compound with prolonged vasodepressor activity; (4) formation and release of a circulating vasodepressor compound other than a prostaglandin and which is not inactivated by the lungs. Any of these hypothetical mechanisms for the delayed response of blood pressure fall might mediate the prolonged vasodepressor action of prostaglandins A as well. Here, however, the release of any mediator would not necessarily be confined to the distribution sites of the abdominal aorta since prostaglandins A are not metabolized by the lung. It is possible therefore that peripheral arteriolar dilation produced by prostaglandins A (and the delayed effects of prostaglandins E) occurs, at least in part, by an indirect rather than a direct mechanism of action.

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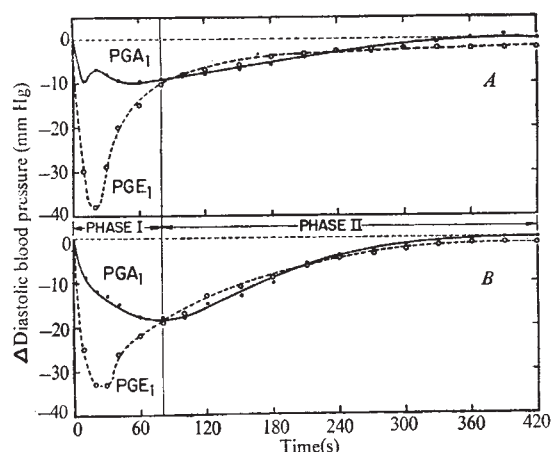


Fig. 3 Time course of blood pressure response following prostaglandin administration. A = Non-pentolinium treated animals, B = pentolinium-treated animals. Each point represents the mean of six animals. Prostaglandin dose: 80 ng intra-aorta.

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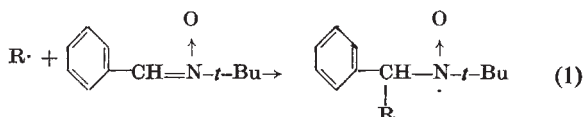


Fig. 1 A, The ESR spectrum of the nitroxide radical from cigar smoke and phenyl-*t*-butyl nitron in benzene. B, The ESR spectrum of the nitroxide by ultraviolet irradiation of a solution of phenyl-*t*-butyl nitron in benzene containing 1% ethanol.

Free Radicals in Tobacco Smoke

THE presence of free radicals in cigarette smoke was demonstrated by Lyons *et al.*^{1,2}, who trapped cigarette smoke condensate at low temperatures and examined it for unpaired electrons using electron spin resonance (ESR) techniques.

Nitroso compounds and nitrones have been reported to be spin traps for short lived species and this technique has been shown to be useful for the identification of the initial radical fragment³⁻⁶. When α -phenyl-*N-t*-butylnitron (PBN) is used as a spin trap, the initial reactive radical R $\cdot$ adds to the nitron to yield the longer lived nitroxide radical (equation 1).



Identification of the radical trapped is derived from the values of the nitrogen and β -hydrogen hyperfine-splitting constant.

A 20 ml. glass syringe with a suitable connexion was used to aspirate the smoke directly from various cigarettes (non-filter), cigars or pipe tobacco. The smoke was then discharged through a 'Teflon' capillary into 0.25 ml. of a 0.05 M solution of PBN in benzene, at room temperature, in a 3 mm ESR cell. After de-aeration by passing a stream of argon bubbles through the solution, the ESR spectra were measured. Usually five 20 ml. smoke samples were aspirated, but in most cases as little as one 20 ml. sample was sufficient to produce a strong signal. Measurements were made at room temperature with a Varian 4500 ESR spectrometer. The sweep rates were calibrated by the spectrum of *p*-benzosemiquinone in aqueous alcohol according to the procedure of Maki⁷.

A quantitative evaluation of the relationship of smoke and the number of radicals was not attempted. But we found that although the visual density of the smoke from cigarettes was greatest and their benzene solutions had the deepest colour, these solutions gave very weak signals. Smoke from cigars gave the highest concentration of radicals.

One 20 ml. aspiration is roughly equivalent to one "puff", which makes this a simple and sensitive technique for the evaluation of radicals from smoke. Benzene solutions to which PBN had not been added and which had been treated similarly with smoke gave no signal on the spectrometer.

The hyperfine splitting pattern was typical for this nitroxide radical, in which the nitrogen triplet is split into doublets giving a six line spectrum⁵. The splitting constants were almost identical for all of the tobacco smokes: $A^N 13.76 \pm 0.12$ gauss and $A^H 1.99 \pm 0.10$ gauss, indicating that the same type of radical was trapped in all cases. Fig. 1A shows an ESR spectrum from cigar smoke. This spectrum also shows a radical which appears as a triplet with 8.01 gauss splitting. This is probably from α -benzoyl-*N-t*-butylnitroxide radical arising from oxidation of PBN^{5,8}.

In our studies of nitroxide radicals from PBN we have noted that ultraviolet irradiation of solutions of PBN in benzene with traces of alcohols gave spectra with hyperfine splitting constants almost identical to those observed for tobacco smokes. In these examples we believe the nitroxide radicals arise from addition of $\cdot\text{OR}$ species to the nitron⁹. In Fig. 1B the spectrum is from the irradiated solution of PBN in benzene containing a trace of ethanol. The splitting constants are $A^N 13.88$ gauss and $A^H 2.09$ gauss. We have also noted these splitting constants from the irradiation of solutions of PBN and diethylmercury or triethyl lead acetate in benzene in the presence of traces of oxygen⁹. When R in equation (1) is an alkyl group, the β -hydrogen splitting constants are of a larger order. For example, when solutions of PBN and diethylmercury in benzene were thoroughly de-aerated and irradiated with ultraviolet light, the splittings we observed were $A^N 14.58$ gauss and $A^H 3.19$ gauss which are characteristic of the nitroxide in which R = Et (equation 1)⁵.

Apparently the short lived species which attaches to the C atom of the nitron may be an $\cdot\text{OR}$ radical. It is not clear why these radicals seem stronger from the cigar and pipe tobaccos.

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Possible Wind Transport of Coffee Leaf Rust across the Atlantic Ocean

COFFEE leaf rust (*Hemileia vastatrix* Berk. and Br.), for long widespread in Asia and Africa, can devastate crops of *Coffea arabica*. The fungus spread rapidly in West Africa between 1952 and 1962^{1,2}, but the New World seemed free from it

until 1970 when it was found scattered over a wide area in Bahia, Brazil, where its consequences may be serious^{3,4}. Indeed, in an attempt to protect the valuable crop farther south, a quarantine belt is being established 500 miles long and 30 miles wide from Rio de Janeiro to beyond Belo Horizonte in which all coffee trees are being destroyed.

and set up many scattered foci of infection. The region is subject to seasonal heavy rain which, if coincident with spore transport, would deposit the spores and aid infection of the coffee.

Given the possibility of long distance transport by wind, West Africa north of the equator is an unlikely source because

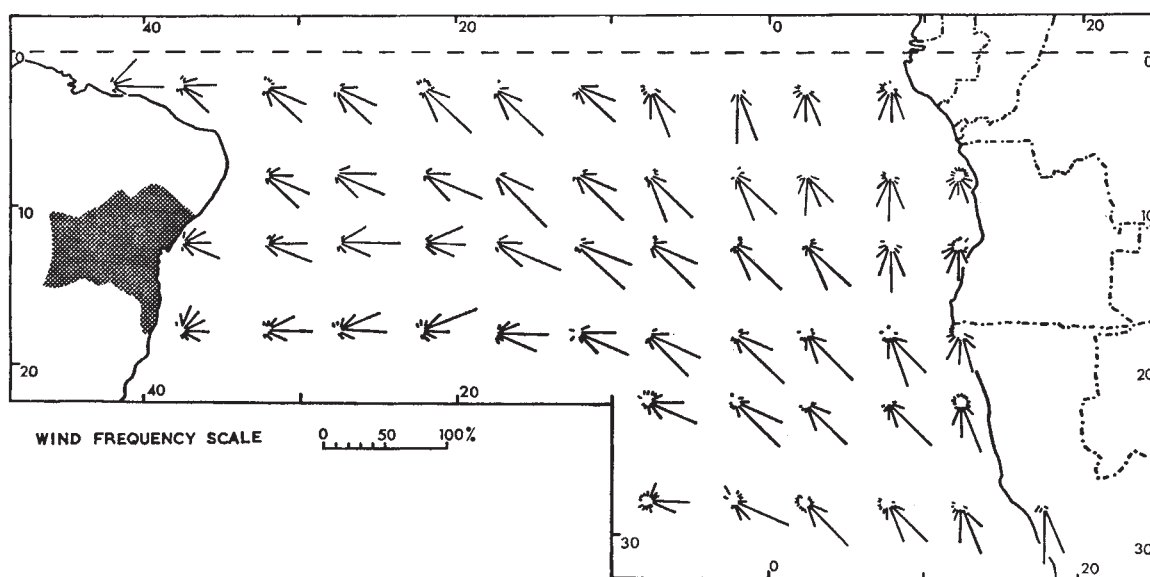


Fig. 1 Percentage frequency of surface winds between south-western Africa and Bahia, Brazil (shaded area), in January for 61 yr. (After ref. 12.)

How the pathogen reached Brazil is unknown. Carriage by wind has been thought unlikely, because the falling speeds of the uredospores of *H. vastatrix* seemed too great to allow long distance transport, and because relatively few spores have been detected by spore traps above affected coffee trees. Rain-splash has been suggested as the principal means of spread⁵. The pathogen could have reached Brazil on plant material, or been imported in some other way by human activity, but we consider that the possibility of wind transport should be reconsidered. If our suggestions are correct, the quarantine belt may prove ineffective and there is danger that the disease may continue to spread by wind in Brazil and elsewhere.

The measurements with *H. vastatrix* uredospores that seemed to exclude wind transport gave terminal velocities of 20 cm s^{-1} for clusters of spores and $7\text{--}10 \text{ cm s}^{-1}$ for single spores⁶. These values are approximately ten times greater than those measured by other workers for uredospores of cereal rust fungi which are of similar size. Measurements made by one of us (P. H. G.) in cooperation with Dr C. E. Hislop at Long Ashton Research Station suggest much slower velocities. Partition of single uredospores between the first and second stage of a cascade impactor⁷ indicated a terminal velocity of 0.6 cm s^{-1} and a total count, including clumps, gave a maximum value of 1.2 cm s^{-1} . The earlier values were clearly in error, possibly because of a bulk sedimentation artefact⁸. Therefore the uredospores of *H. vastatrix* seem just as liable to be carried long distances by wind, once they are airborne, as are those of cereal rusts. There is evidence that such rust uredospores can make journeys lasting up to one week^{9,10}.

The disease was discovered, in Brazil, within a few weeks in 1970 at many scattered places in a region measuring 700 miles long by 300 miles wide. A recent survey indicated that the rust had been in Bahia for 4 or 5 yr, spreading locally before it was discovered¹¹, and it was assumed that the rust had been introduced by man³. An alternative hypothesis, however, is that airborne spores crossed the South Atlantic

continuous winds from the surface to about 10,000 feet do not blow from the region of the Gulf of Guinea to South America (Fig. 1). By contrast, trade winds flow frequently and for long periods at these heights from the Atlantic Ocean off the shores of Angola, where the disease was reported in 1966¹², into the affected areas of Brazil especially during January to April at speeds which could enable coffee rust uredospores, if wafted out to sea by off-shore winds, to travel to Bahia in 5 to 7 days¹³ (Fig. 1).

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Cortical Source Locations of Pattern-related Visual Evoked Potentials recorded from the Human Scalp

ALTHOUGH there have been many reports about human averaged evoked potentials to visual stimulation, the relation of these scalp-recorded potentials to their underlying sources is obscure, although stimulus-related visual evoked potential (VEP) components are generally thought to originate in the visual cortex. Most cells in the visual cortex of animals such as the cat and monkey are contour-sensitive¹⁻³, and so studies involving patterned or contoured stimulation seem most likely to provide such information. I report here some preliminary results which illustrate the great influence of the location of a retinal stimulus on the characteristics of pattern-related VEPs, and indicate how such studies provide clues to the probable location of the sources of different components of these VEPs.

Although there have been a few reports of changes in the waveform of VEPs obtained by flash stimulation of different regions of the (homogeneous) visual field^{4,5}, there is increasing evidence that retinal location has a profound influence on both waveform and spatial distribution of various pattern-related VEPs⁶⁻⁹. Polarity reversal of corresponding components of the monopolar VEPs produced by stimulation of corresponding regions of the upper and lower visual field has been reported, both for the presentation of a stationary pattern into a previously homogeneous field^{7,10} and for an alternating chequerboard field^{8,9}.

I have investigated the changes of waveform and longitudinal amplitude distribution of the binocular VEPs produced by the presentation of pattern into various central and annular regions of the upper and lower half of a steadily illuminated field^{7,10}. A four-field tachistoscope provided the alternately homogeneous and patterned 6° circular stimulus field, a continuously lit surround and a dim central fixation cross. Partially patterned transparencies provided patterned stimuli of the required spatial configuration, the pattern being a chequerboard of hollow 14 foot squares of line thickness 3.5 feet. The viewing distance was 1 m. The brightnesses of the homogeneous regions of both test fields and of the surround were subjectively matched, the luminance level being typically 10 milliLamberts. The patterned field duration was 25 ms and the aperiodic interstimulus period varied between 0.5 and 0.8 s. A CAT computer and a fourteen channel frequency modulated tape recorder were used in the computation of averaged responses recorded monopolarly, with reference to the right ear lobe, from a midline row of ten electrodes of 2.5 cm apart, the lowest but one electrode being on the occipital protuberance. (The contribution of the ear reference electrode to the monopolar VEPs was found to be insignificant.) Three averaging runs of fifty stimulus presentations were carried out for each stimulus condition for four subjects, the averaging duration being 0.5 s.

The polarity inversion and different amplitude distributions of corresponding components of the upper and lower field VEPs are illustrated in Fig. 1, which shows the initial 0.25 s of the three superimposed VEPs to the presentation of the pattern into (a) the upper and (b) the lower half of the stimulus field for one subject (D. A. J.). The upper field VEP is triphasic with negative components of peak latency about 80 and 180 ms, and a positive peak at about 110 ms, while the lower field VEP has peaks of similar latency but of opposite polarity. The close similarity of waveform between VEPs to full field stimulation and that of the sum of the separate half-field VEPs can be seen in Fig. 1d and c.

Studies of the influence of pattern defocusing and pattern duration have shown that the initial biphasic part of these half-field VEPs is caused almost entirely by the appearance of the pattern. The third peak can, however, contain significant

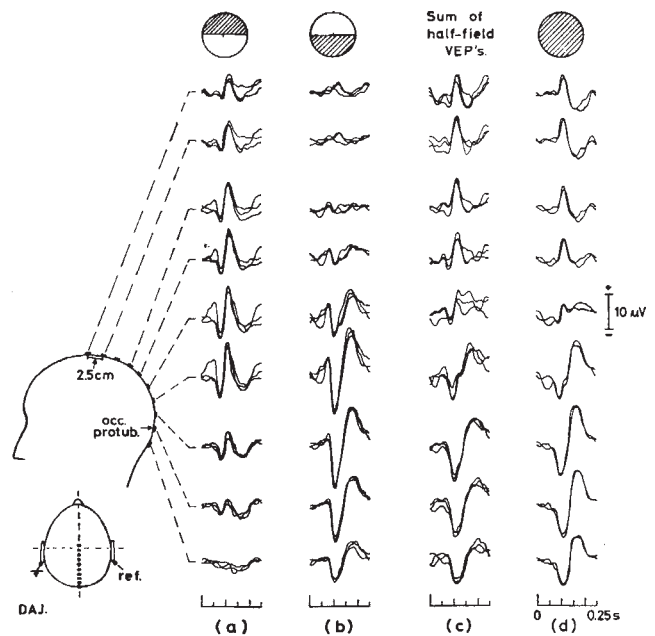


Fig. 1 Monopolar VEPs recorded from a midline row of electrodes for one subject (D. A. J.) to patterned stimulation of (a) the upper half of the 6° (diameter) stimulus field, (b) the lower half of the stimulus field, and (d) the full stimulus field. The waveforms of the sum of the upper and lower field VEPs are shown in c. (Here and in Fig. 2, the shaded area within the circles denotes the patterned region of the stimulus field.)

contributions related to the disappearance of the pattern and possibly other factors, and so is disregarded for the moment. A comparison of the longitudinal distributions of the two initial peaks of the upper and lower field VEPs shows that whereas the first peaks have similar (but opposite polarity) distributions, those of the second peaks are very different for three of the subjects. Plots of the midline distributions of the mean amplitudes of the second peaks of the upper and lower field VEPs for all four subjects are shown in Fig. 2a. The amplitude distributions for subjects D. A. J. and A. J. have similar characteristics to those observed for the upper and lower half-field VEPs of eight of twelve subjects in a previous study¹⁰. Here, the lower field VEP distribution is fairly symmetrical with a maximum 2-5 cm forward of the occipital protuberance, decreasing towards zero amplitude in both posterior and anterior directions. The upper field VEP distribution is more diffuse and asymmetrical, with a maximum 5-10 cm forward of the occipital protuberance and a more rapid attenuation in the posterior than in the anterior direction; significant amplitudes are recorded from vertex electrodes. The distributions for J. C. are fairly similar and diffuse, while those for S. H. illustrate the only case so far encountered where these VEP components have similar polarity. For this subject, the lower field VEP distribution has the same polarity (negative) as those of the other three subjects, but the upper field VEP distribution is negative and sharply localized at the posterior electrodes and changes polarity to become positive at the anterior electrodes.

These differences in the characteristics of the first and second peaks can be explained in terms of dipole-like potential fields associated with the topographically dissimilar regions of striate and extrastriate cortex, respectively, thought to represent the upper and lower regions of the visual field. The limited direct experimental evidence¹¹⁻¹³, as well as phylogenetic considerations¹⁴, suggests that the human visual cortex has a basically similar retinotopic organization to that of the monkey¹⁵⁻²¹. On this basis, the upper and lower visual field quadrants are represented in regions of striate cortex below and above, respectively, the contralateral calcarine fissures, with the horizontal meridian represented in the depths of the fissures¹¹.

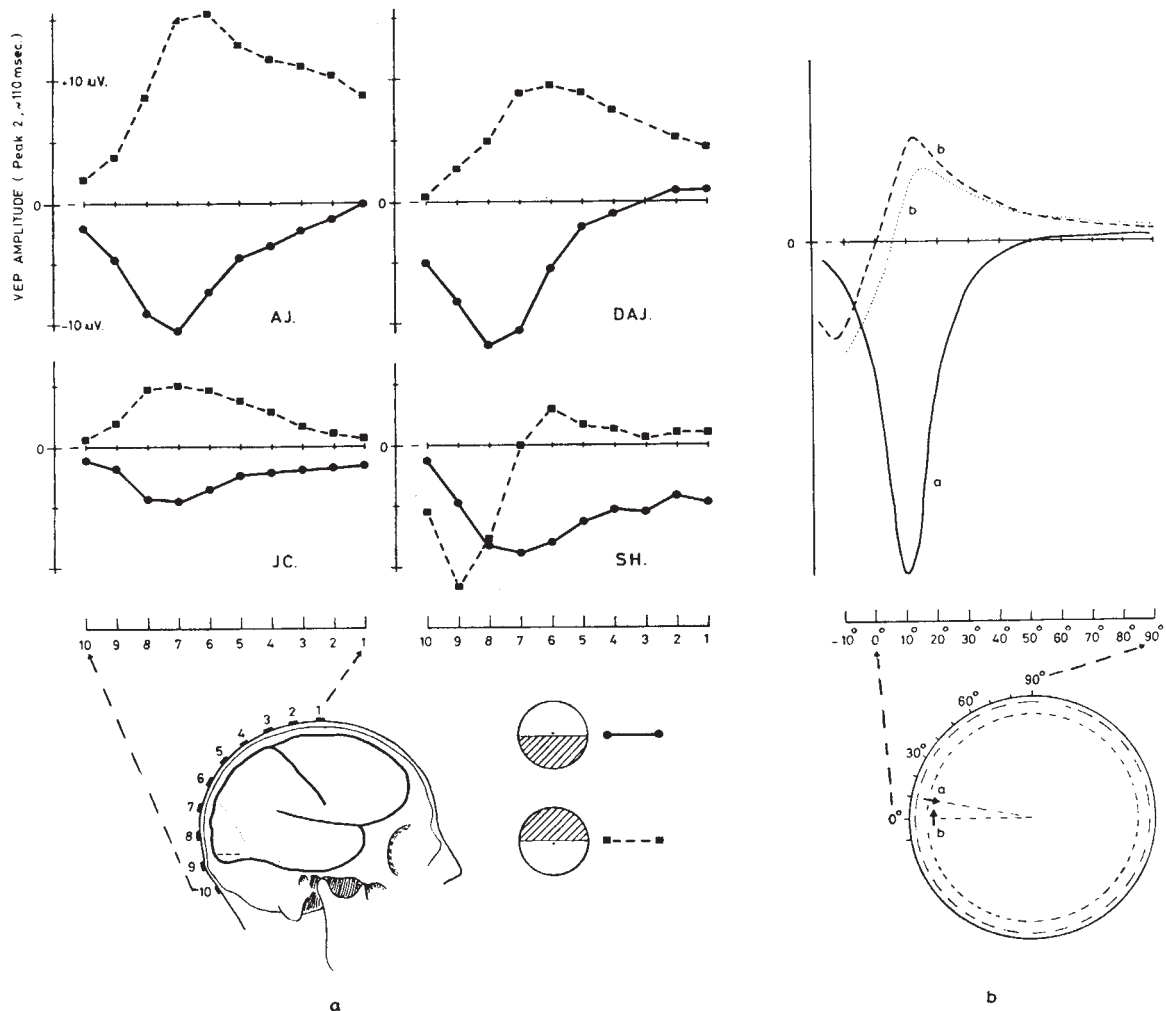


Fig. 2 *a*, The midline amplitude distributions of the second component peak of the VEPs to stimulation of the upper (---) and the lower (—), for the four subjects. Estimated error is about $\pm 1 \mu$ V. The lower sketch gives a rough indication of the relation of the surface electrode positions to the underlying visual cortex (occipital lobe bounded by the dotted line), and to a proposed location of the calcarine fissure (---). *b*, Potential field distributions at the surface of a homogeneously conducting sphere due to a single dipole at a depth $0.8 R$ (R is sphere radius) and oriented perpendicular to the surface (—), and when displaced posteriorly by 10° and at a depth $0.75 R$, and oriented parallel to the surface (---), and at 15° to the surface (...).²⁶

Similarly, upper and lower visual field areas are represented in those regions of extrastriate cortex on the external lobe surfaces below and above, respectively, the level of the horizontal meridian representation in striate cortex^{19,20}. If the posterior extremity of the calcarine fissure lies close to the occipital pole (Fig. 2*a*), most of the extrastriate cortex below the level of the fissure, that is, the portion which is thought to represent the lower visual field, lies on the under lip and surface of the occipital lobes, whereas the lower visual field representation lies mainly on the upper surfaces.

A very crude model of the activity of the cortical regions on the upper and under surfaces of the occipital lobes consists of single dipoles inside a homogeneously conducting sphere whose orientations are perpendicular and parallel, respectively, to the surface (Fig. 2*b*) (ref. 26). (There is increasing evidence that the cortical generators of surface-recorded EPs have dipole-like characteristics, where the equivalent dipole axis is perpendicular to the cortical surface²²⁻²⁵.) This model shows that the basic differences in shape between the surface potential distributions of the perpendicular (*a*) and the parallel dipole (*b*) correspond to those observed for the lower and upper field VEPs, respectively, for subjects A. J. and D. A. J. (Fig. 2*a*). Moreover, the uncommon form of the upper field VEP distribution observed for subject S. H. (Fig. 2*a*) can be explained if, in this case, the posterior extremity of the calcarine fissure is some distance above the occipital pole, so that a greater portion of extrastriate cortex representing the central upper

visual field lies on the upper surface of the lobe. (The calcarine fissure configuration is in practice extremely variable¹⁴.)

Similarly, dipole-like distributions associated with the regions of striate cortex forming the "floor" and "roof" of the calcarine fissure can explain the similar (but opposite polarity) amplitude distributions of the first peak of the upper and lower field VEPs, respectively, as well as the opposite polarity of the initial two components of the same half-field VEP.

Some results obtained for stimulation of central and annular regions of the stimulus field are shown in Fig. 3. It was found that stimulation of successive annular regions from the centre outwards produced a progressive anterior displacement and widening of the lower field distributions (Fig. 3*a*), but no comparable displacement or change in form of the upper field distributions for the same subject (D. A. J.) although there was more rapid attenuation of amplitude with the outer annular stimuli (Fig. 3*b*). The amplitude distributions for both upper and lower half-field stimulation were similar in form but smaller in magnitude than those of the sums of the distributions obtained to stimulation of their constituent annular regions. For subject S. H., the form of the distributions obtained to stimulation of central and annular regions of the upper visual field changed systematically from being wholly negative for the central 1° stimulus to being wholly positive for the outer annular (2.0° – 2.5°) stimulus (Fig. 3*c*). These results suggest that progressively anterior regions on the upper surfaces and progressively posterior (or deeper) regions on the under sur-

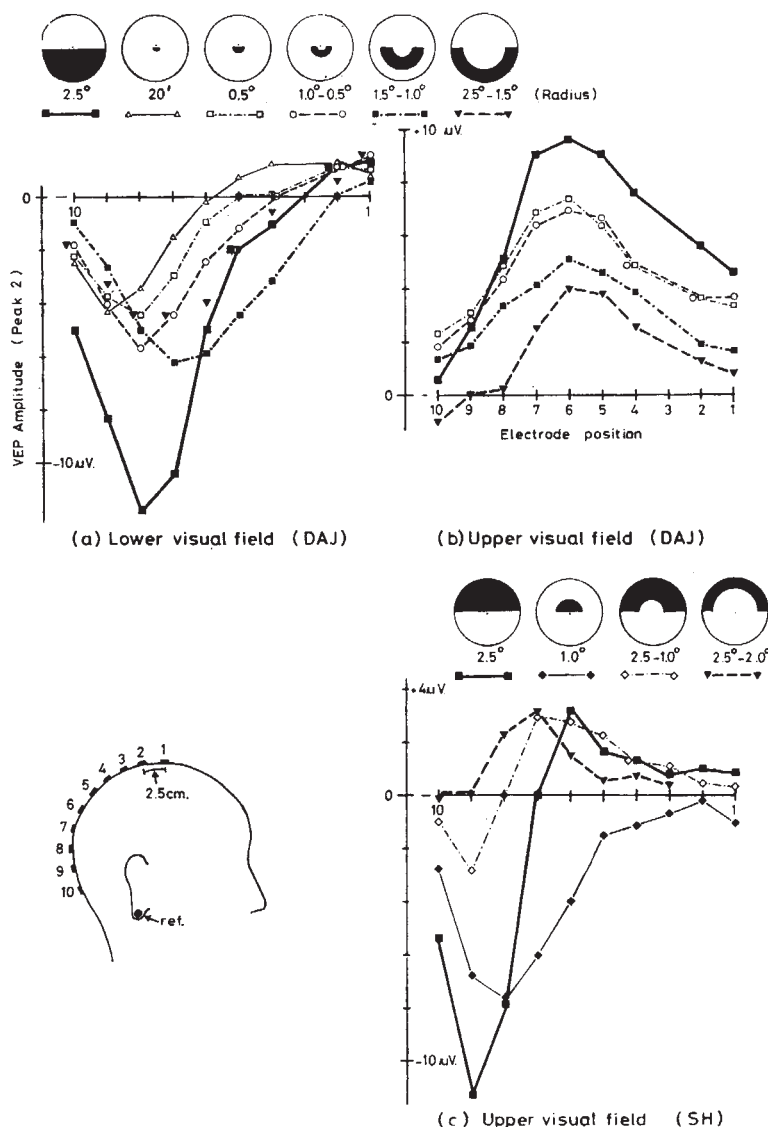


Fig. 3 Longitudinal midline amplitude distributions of the second component peak of the VEPs obtained to stimulation of successive central and annular regions (indicated by the dark areas within the circles) of (a) the lower stimulus field and (b) the upper stimulus field for the same subject (D. A. J.), and (c) the distributions obtained for stimulation of central and outer annular regions of the upper stimulus field for subject S. H.

faces of the lobes are activated when the stimulus is moved away from the centre into the lower and upper visual field, respectively, and are consistent with the data on retinotopic organization of the monkey's extrastriate cortex^{19,20}.

To summarize, I propose that the first peak in these midline "pattern-appearance" VEPs is produced chiefly in the striate cortex lying within the calcarine fissure, whereas the second peak is produced chiefly in the extrastriate cortex on the external surfaces of the occipital lobes. Both components are thought to reflect cortical activity of a similar (surface-negative) nature, the observed surface VEP variations being a consequence principally of the differences in topography of the associated cortical regions. These conclusions contrast with those of Halliday and Michael⁸, who proposed different forms of cortical activity to explain the observed polarity reversal of the upper and lower field VEPs produced by alternating chequerboard stimulation.

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BOOK REVIEWS

An Attitude Explored

The Cambridge Mind: Ninety Years of the Cambridge Review, 1879-1969. Edited by Eric Homberger, William Janeway and Simon Schama. Pp. 315 + 18 photographs. (Jonathan Cape: London, 1970.) 80s.

It would be surprising if an anthology of articles from the *Cambridge Review* collected to mark its ninetieth anniversary did not contain some interesting material; and it would be interesting to see whether the *Oxford Magazine*, now alas defunct, could provide as wide a range of what are, chiefly, book reviews and obituaries. The latter might help one to decide the more difficult question suggested by the title; is there in fact a "Cambridge Mind"? The editors suggest some of its possible characteristics: "a rigour of logical analysis; an uncompromising exercise of sceptical inquiry; a commitment to verification rather than imaginative construction" (p. 16). But one would think that these would be essential components of the mental world of any great university, not just of Cambridge. The editors go on to say that "the same severity of standard is applied to the function of language in poetry as it is to the explanation of genetic information or to the study of seventeenth century society". The difficulty there is to find a reviewer competent to decide on the basis of this volume whether this is true or not. If these were the characteristics of Cambridge history, Professor Godfrey Elton, in the course of an illuminating piece on Maitland, tells us that this is no longer generally true; and T. S. Eliot, writing in 1928, perhaps not with Cambridge especially in mind, was lamenting the arrival of the age of the amateur and of the fact that there was by then "very little respect for authority; by which I mean respect for the man who has special knowledge of some subject of which oneself is ignorant" (p. 227).

Because I do not have the capacity to judge the section dealing with the natural sciences, I am prepared to take the editors' word for it; and in any event the glories of the Cavendish and other Cambridge centres of scientific excellence hardly need emphasizing. But when one looks at the "social sciences", the editors' claim becomes

harder to maintain. There are some good destructive pieces in what one hopes is the true Cambridge vein: Michael Oakshott on "Dialectical Materialism" and John Dunn on Hannah Arendt. But were the editors really kind, in relation to all that has happened and is happening in Soviet Russia and Western Europe, to reprint a piece by Professor Joan Robinson, dated 1937, in which she argued that under socialism the entire process of production and distribution could be carried out without the intervention of a pricing system except where, everything else having been provided, it was just a question of "choosing between one kind of luxury and another". How much of a commitment to "verification rather than imaginative construction" went into that? And one would think that the late Bertrand Russell to whom the volume is dedicated might have been more worthily represented by his philosophical or mathematical work than by his amateur dabbings in international politics.

The editors claim no particular virtues for Cambridge as a nursery of stylists; yet as one reads through a volume like this, one cannot help but be impressed by the variations in the capacity for expressing thought both over the years and as between the different disciplines. The best written piece is the first, a letter from India written by Archdeacon Cunningham in 1882; though Mr F. L. Lucas's obituary of Julian Bell, published in 1937, has a fine passage of satire at the expense of Franco and his friends. The scientists wrote effectively, and the historians also. On the other hand, the great reputation of the Cambridge English school would not seem to be based on any capacity of its members (Mr Lucas alone excepted) to say clearly what it is they have in mind.

Perhaps a Cambridge quality that explains this defect is the quality of other-wordliness which can so easily degenerate into an insular snobbery. It is clear that Lord Snow felt this when in reviewing H. G. Wells's splendid autobiography he wrote: "It is probably fair to say that various bodies of 'Cambridge' opinion have shown more unanimity in disapproving of Wells's work than in anything else whatever" (p. 281).

It would be hard to pick up this volume without finding something one was pleased to read; but if there is to be a study of the Cambridge mind, it will have to be based on the asking of questions, not on the talents of three anthologizers.

MAX BELOFF

Educational Frontiers

A Strategy for Education. By Herman T. Epstein. Pp. vii + 122. (Oxford University: London, December 1970.) 35s.

It would be a shame to shrug off this little book with a disdainful smile, though in some ways it asks for it. Under the pretentious title we find a recipe for teaching science to non-specialists. The outrage to accepted practice is obvious, the evaluation incomplete. Not much attention is paid to what others have done or written on teaching science—not even to something so very close to the author's principal theme as Michael Yudkin's *General Education* (Allen Lane, 1969). And yet there is food for thought here: a constructive proposal to meet an urgent need from an enthusiastic pioneer.

Epstein's approach, the "research studies method", was developed initially for teaching biology to non-science students at Brandeis University. Each teacher chooses, from his own research field, six to ten original papers that make up a single "story line". Classes consist of a series of seminars on these. It is claimed for the method that it reverses the "flight from teaching" of research scientists; apart from selecting the set of papers, they are supposed to require little preparation time, for the material is already very familiar to them. Background explanations are given in digressions of not more than ten minutes. Use of textbooks, reviews and *Scientific American* articles is discouraged during the first two thirds of the semester long course, because they shut off discussion; classes are conducted through student questioning of the teacher. "The students should be helped to learn to rely on their instructors for answers to questions" (p. 37). Despite this, Epstein claims that learning is "experience-based". It all depends,

of course, on what you mean by "experience".

The emphasis throughout is to be kept away from the information content of the papers so that it can be focused on the activities of scientists in getting the information. Students should "go through the acquisition of the insights by the working scientist" (p. 35). Success is claimed in getting classes to propose new experiments. But is the method really superior to Yudkin's suggestion of laboratory-based investigation of much more elementary problems? Or to the historical case study method developed at Harvard by Conant, which is dismissed quickly and quite unfairly?

Epstein admits that some of the success of his courses might be attributable to small classes and to a sort of Hawthorne effect, enthusiasm generated by sheer novelty. His own enthusiasm may have carried him over the edge of objectivity. The honour grades attained by most of the students are used as evidence of success: dubious evidence, given that the suggested basis for grading is that those who "participate at all actively are likely to be given honour grades", while "attendance at the majority of class meetings will almost surely suffice for passing the course" (p. 44).

Some of the limitations and drawbacks of the approach are obvious enough. Balance and breadth may well become a matter of pot-luck. There are as many different courses as there are teachers. From them, the students are expected to acquire something about the strategy and the tactics of the activity called science—presumably in a tacit form, for Epstein does little to make it explicit. The method is intended chiefly, though not exclusively, for short courses not leading on to further work in the subject. Examples are given of its use in biology and economics—but what about fields like organic chemistry or electromagnetism? "The screening of potential instructors was mainly on the ability to feel comfortable when making up relatively superficial explanations for matters that could not be clarified on the basis of readily describable experiments" (p. 29). Eyebrows will be raised—and not only in the more exclusive Black Paper circles—at the sort of explanations that arise. How much of this is education, and how much is facile titillation?

There is plenty of scope here for withering sarcasm from the experts—but the fact remains that Epstein has shown that the approach can be made to yield stimulating educational experiences. Gentle reader, why not try it? Is there really much to lose? Attempts to teach non-specialists have foundered all too often in the past on

criticisms by fellow specialists. Much harm has been done thereby to education. Better, perhaps, to play for a change to the gallery of students rather than the gallery of professional colleagues.

F. R. JEVONS

Memoir of Early Science

Simon Stevin: Science in the Netherlands around 1600. By E. J. Dijksterhuis. Pp. vii+145. (Martinus Nijhoff: The Hague, 1970.) 27 guilders.

THE appearance, after a delay of some years, of the late Professor Dijksterhuis's book on Simon Stevin is indeed a welcome event. Stevin, who was born at Bruges in 1548 and died in Holland in 1620, was for over forty years a dominant figure in northern European science, mathematics and technology. Though he does not rank as one of the giants of early modern science, he was an important and influential figure for a variety of reasons. This first substantial monograph on him in English should do much to enhance his reputation and to make his contributions better known not only to historians of science but to other interested readers as well.

Like all of Dijksterhuis's work, *Simon Stevin* bears the stamp of authority and competence. It was originally written as an abbreviated version of his Dutch work of the same title. Being written in Dutch and published during the war (1943) that work has had, understandably, little diffusion. At Dijksterhuis's death in 1965, the condensed English version was nearly ready for publication. It has now been brought through the press by R. Hooykaas and M. G. J. Minnaert. The volume is certainly an excellent introduction to Stevin's life (of which very little is known) and scientific work, but although said to be "adapted to the educated, non-specialist reader" it is still somewhat technical in parts. It seems to me to suffer from being overly condensed. Dijksterhuis certainly had an intimate knowledge of Stevin's works and could easily have written a book of twice or three times the length. This would have made it somewhat more accessible to the non-specialist and would have allowed the author to expand on some of the many fascinating topics which he has been able to treat often only in the most sketchy fashion.

All things considered, however, the book is an excellent general introduction. The appetites of many readers should be whetted by it, and they can now dip into Stevin's own writings just recently available in an excellent critical edition with English translation (published in Amsterdam, 1955–66, in five volumes). One is struck in reading this

volume by the remarkable range of Stevin's interests and attainments. Not only did he write on mathematics, mechanics, and navigation, subjects in which his contributions are well known, but on many other topics as well, including architecture, music and city planning. It is interesting to compare his work with the perhaps more highly developed Italian science of the period. If some ideas of Benedetti, for example, come to him through Taisner, in other matters, his experiment with falling bodies (1586), say, he seems to have been in advance of the Italians. Stevin was also among the earliest to write important scientific treatises in the vernacular. Though his essay in favour of Dutch as an excellent language in which to write science had little influence, some of his arguments are still of interest and worthy of further study. One notes that his *clootkrans* (wreath of spheres) by which he demonstrated the law of the inclined plane has become the symbol of the new *Dictionary of Scientific Biography*.

Like all of Nijhoff's productions this volume is handsomely produced and has well executed drawings and diagrams, besides several well chosen plates. The English is generally readable and clear and one notes few infelicities of style. It has a useful brief list of Stevin's works, along with the most important editions and translations of them. One could have hoped for a more complete list of secondary literature on Stevin, for only four items are listed. The index is well done and useful.

C. B. SCHMITT

Drug Story

Hallucinogenic Drug Research: Impact on Science and Society. Edited by James R. Gamage and Edmund L. Zarkin. (Proceedings of the First Annual Symposium of the Student Association for the Study of Hallucinogens.) Pp. xvi+139. (Stash: Beloit, Wisconsin, 1970.) \$2.95.

THE blurb of this book claims that "people, in learning about drugs, must not only study what doctors, pharmacologists, psychologists and other authorities have to say, but must bring themselves up to a level of competence which will permit critical evaluation of the validity and relevance of the information that comes into their hands". This proposition is certainly true, but I cannot help wondering if the contents of this book are likely to make the necessity come to pass.

But this need not imply that this is a bad book. Indeed, it is an extraordinarily useful one, but its use does not lie in the direction indicated so misleadingly (perhaps deliberately) by the cover. In fact, rather than "Impact

on Science and Society", the subtitle might better be stated as "An Attempt to Educate Certain Individuals". Perhaps I am reading too much into the book, but it seems that this was a subtle and responsible attempt to do just that. The difficulty is that the individuals at risk, even if they are science students, are not in this matter students of science. So, although it would be a waste of time to emphasize the paraphernalia of science, the conclusions must be scientifically impeccable; and sensationalism, if it is permitted at all, had best come down in favour of the defendant drug rather than against it. This is what happens here; nevertheless, my chief anxiety in reviewing the book in these terms is that I may have unwittingly blown its "cover". Most of the science, in fact, is confined to the glossary, which is excellent. "Efficacy," for example, is simply defined as "effectiveness"; and the definition of "malvaria" begins "Purportedly"—a good beginning for a definition in a volume that contains a contribution by Abraham Hoffer himself. The other, more usual kind of glossary is of course inappropriate for this audience. In fact, the only traces of student humour are to be found in the name of the publishers ("Stash" also means Student Association for the Study of Hallucinogens, the body that sponsored this symposium), a cover that is got up to look rather like something similar from the National Institute of Mental Health, and the dedication in minute type to the Attorney General of the United States and his wife for their efforts to further America's "enlightened drug policies".

The list of contributors is impressive, and the way they make their points doubly so. Solomon Snyder, for example, quotes the 0.1–0.2 per cent "bad trip" and the 20 per cent flashback rates; Joel Fort notes that "The best way for a society to deal with drugs is to civilize itself"; Daniel Freedman: "The TV show in the head" experienced by Indian peyote users is considered a distraction and not the purpose itself; Stanley Krippner, quoting Plato: "New modes of music herald upheavals in the state"; Hoffer giving unorthodox but interesting advice on how to avoid bad trips; Samuel Irwin, and others, emphasizing chromosome damage as a great potential danger in spite of the inconclusiveness of the evidence, but also giving due weight to the "psycho-social dangers" of hallucinogen taking.

The overall temperature is that of a cool scene; the hot hallucinogens like LSD are, in fact, firmly put down. What is still as badly needed as ever, of course, is a controlled comparative trial of the effectiveness of different means of education: I wonder if the bushfire reactions of those hardline

professors who stamp about on public platforms are even half as effective as this kind of student soft sell.

C. R. B. JOYCE

Child Development

The Child in His Family. (The International Yearbook for Child Psychiatry and Allied Disciplines, Vol. 1.) By E. James Anthony and Cyrille Koupernik. Pp. xxxii + 492. (Wiley-Interscience: New York and London, October 1970.) 120s.

THIS collection of papers, prepared for an international congress on child development, includes work from a number of different viewpoints loosely organized around themes: family dynamics, family vulnerability and crisis, chronic family pathology and studies of families in different cultures. It is the first volume in a yearbook series. Contributions from specialists trained in various behavioural science disciplines add to our knowledge of the complex interactions of family life, culture and personal development. They vary in scope from Lidz's broad discussion of "The Family as the Developmental Setting", to specific reviews and studies such as Bowlby and Parkes's "Separation and Loss within the Family". Anthony's study of the impact of serious mental and physical illness in a parent on family life, and Greenberg's discussion of atypical infant development in relation to the behaviour and personality of the mother amplify and document clinical impressions. Studies of child rearing in Indian, Turkish, African, Sudanese and isolated Norwegian mountain families add to our sense of the range of variability of personality development in response to differences of culture and ecology. In various ways they indicate the value of getting beyond the classical psychoanalytic categories in studying the complexities of human development.

LEONARD J. FRIEDMAN

Fibroblastic Disorders

The Differential Diagnosis of Fibroblastic Disorders. By D. H. MacKenzie. Pp. ix + 167 + 88 plates. (Blackwell Scientific: Oxford and Edinburgh, 1970.) 55s.

DR MACKENZIE's monograph on fibroblastic disorders is an expansion of a Hunterian lecture and has both the merits and defects of such communications. The field of interest is arbitrarily restricted, so that histiocytomas and elastofibroma come within the author's fibroblastic spectrum, but not the fibrous proliferation of nerve sheaths, while treatment and prognosis are dealt with in a most cursory manner.

After a brief account of granulation tissue, MacKenzie considers the various types of inflammatory and hyperplastic

fibrous conditions, as well as benign and malignant tumours; each section is illustrated by photomicrographs of good quality and a list of references to relevant papers which will be useful for those who seek a sufficiency of information for the various higher examinations that effectively impede the training of consultants. For the practising surgeon MacKenzie's book will be disappointing as there is little about the natural history of the conditions dealt with, nor will the young diagnostic pathologist gain as much assistance as he would expect from a book concerned with differential diagnosis. If a condition has been named one can use the text for confirmation or thumb through the illustrations, hoping to find an example similar to that seen under the microscope. The problem of differential diagnosis of connective tissue proliferation is far wider than is covered here, embracing vascular tissue, smooth muscle, nerve sheath and fat as well as fibrous tissue and histiocytes, while on occasions anaplastic epithelial conditions may cause confusion; it is a difficult and important problem, for both prognosis and treatment are dependent on accurate diagnosis.

The deficiencies of this expensive little monograph are all the more regrettable as MacKenzie has a wide experience of the conditions which he describes; if his material had been arranged in a different manner and very slightly expanded it could have been a valuable contribution to pathological knowledge.

A. H. T. ROBB-SMITH

Brain Forms

Catalogue of Mammalian Brains. By Kurt Brauer and Wilfried Schober. Pp. 20 + 150 plates. (Fischer: Jena, 1970.) 173 marks.

IN recent years, a concentration of neuroanatomical research on fine structure and function has led to a preoccupation with the relatively few mammalian brains which can be obtained in considerable numbers and with animals which can be maintained in laboratories for experimental purposes. The very wide range of morphological variation in the brains of different mammals has attracted little attention, and one can welcome the publication of this catalogue as doing something to redress the balance between different interests.

It consists of excellent photographs, each accompanied by an explanatory line drawing, showing the brains of about 150 mammalian species distributed over twelve of the eighteen orders for which living representatives exist. The specimens are in the Brain Research Institute at Leipzig, but future instalments are promised which will illustrate additional specimens in other institutions. As the present volume is

bound in loose leaf form, these additions can be clipped in as they become available. So far the Chiroptera, Primates, Rodentia, Carnivora and Artiodactyla are well represented, the other orders usually by three or four species, but the important Marsupialia by only one. Among the omissions, the cetacean is probably the most important order which has no representative.

The specimens all seem to be well preserved and with little distortion or damage. The standard of photography and reproduction is high. Ten linear dimensions (brain length, height and so on) are given for each specimen, and also brain and body weight, the sex (when known) and the method of fixation. Labelling is limited to quite obvious macroscopic structures and will be useful chiefly in relation to sulci and gyri in those brains where such features are evident. The short introductory text is given in both German and English, and a latinized terminology, internationally understood, is used for the parts of the brain. There is an extensive bibliography classified in sections for each of the mammalian orders.

This illustrated catalogue will be of value to taxonomists and, as Professor Stark points out in his *Geleitwort*, particularly to those interested in the evolution of brain morphology. In this context its value is increased by the fact that the brain is the only non-skeletal organ the form and subdivisions of which can be assessed, often with considerable accuracy, in fossil material from endocranial casts. It will provide also a necessary basis in macroscopic morphology for anyone beginning a study in an unfamiliar brain or indeed in comparative neuroanatomy in general. It is to be hoped that the additions needed to make it more complete will soon become available.

F. GOLDBY

Insect Haemocytes

The Cellular Defence Reactions of Insects. By George Salt. (Cambridge Monographs in Experimental Biology, No. 16.) Pp. vi+118. (Cambridge University: London, October 1970.) 50s; \$7.50.

APART from the occasional devastating epidemic, insects usually seem to the human observer to enjoy excellent health. They thrive in a variety of highly contaminated environments, and they can withstand surgical operations of great severity without benefit of antibiotics. And although many viruses, bacteria, fungi and insect parasitoids can attack insects, these infections are often successfully thrown off. Their resistance is of two kinds. Immune responses, involving the formation of antibodies, are unknown in insects. Yet there are less

specific serum responses which are of importance. These are to furnish the subject matter for a future monograph in this series. The present book describes, in elegant prose, the second line of defence: the defence interposed by the blood haemocytes to objects, animate or inanimate, that come to invade the haemocoel.

Smaller foreign particles entering the body cavity suffer phagocytosis. Larger masses such as a hymenopterous parasite of the "wrong" species or an implanted fragment of glass, are rapidly invested in a capsule of blood cells. This intriguing process, which has been studied experimentally by Salt over a number of years, involves much that is likely to interest the cell biologist as well as the entomologist: the adhesion of the cells to the "non-self" substratum; the secretion of extra-cellular matrix—perhaps a mucopolysaccharide—which facilitates cohesion; and finally, the striking flattening process that affects the cells arriving later on the capsule wall.

Why do the cells adhere in the first place? Salt concludes that "the wide range of particles and foreign bodies to which insect blood cells adhere can have no positive characteristic in common; they can only be alike in lacking something". Thus the tendency to adhere to surfaces is regarded as a general property, the failure to adhere the exception. Looking at the problem this way round avoids the complexities associated with "specificity" in vertebrates. Nevertheless, the exceptional surface properties which prevent the lodgment of haemocytes are still of great interest. Circulating haemocytes, the autologous connective tissue layer that lines the haemocoel and any parasite eggs or larvae adapted to the host species in question all possess this property. What this "something" is remains to be decided. But the problem is now neatly framed and awaits further investigation. The whole matter is placed in perspective in a final chapter in which insect and vertebrate defence reactions are compared and contrasted.

The voluminous literature on insect haemocytes, their types and functions, is not notable for its clarity. This lucid and stimulating survey is therefore most welcome.

A. D. LEES

On the Ice Field

The Chemical Physics of Ice. By N. H. Fletcher. (Cambridge Monographs on Physics.) Pp. x+271. (Cambridge University: London, February 1970.) 80s.

PROFESSOR FLETCHER is to be warmly congratulated for writing this eminently readable monograph which I can recommend without hesitation. The book is intended principally for graduate students as an exemplification of

the principles of chemical physics by a detailed study of a single material, ice.

The scope of the book is one that has not been attempted before in such detail. The first seventy pages describe the structure of the water molecule, ice 1 h and the high pressure polymorphs. The next fifty pages consider the structure of water and the mechanisms of the growth of crystals by nucleation and from the liquid and vapour phases. After a brief chapter on the thermal properties of the lattice, the closing 100 pages are concerned with the line and point defects in ice, and their role in mechanical and electrical behaviour.

This book confirms my belief that a carefully told exposition, starting right at the beginning, is more satisfying to any serious student than a book covering a wider field but held together with less coherence. In this way we can understand both the limitations and strength of the text. The limitations are that Fletcher's study of inter-related topics leaves the impression that there is little to doubt in "what is now generally regarded as understood and accepted about the chemical physics of ice". The impression is given that if there is a discrepancy between theory and observation, it is probably the experiments which must be improved. Thus, an all too simple picture may seem to emerge from the treatment. Fletcher has noted that the pace of ice physics has accelerated recently, and most of the references in the book are less than ten years old. The most recent work has tended to give a less clear picture, and *Physics of Ice* (Plenum: New York, 1969), the proceedings of the International Symposium at Munich in 1968, could be read with profit by those who graduate from Fletcher's book.

The second purpose of the book is to provide a synoptic view of the properties of ice for those engaged in research in glaciology, cloud physics and other related fields. Certainly reference volumes for glaciologists dismiss summarily the aspects of ice discussed by Fletcher, and so the book should interest them. Fletcher does not attempt to discuss the flow of glaciers (the creep of polycrystalline ice being concluded in half a page), yet correctly spends fourteen pages introducing and discussing mechanical relaxation in ice, as this is intimately related to the unique crystal structure of ice.

SI units would have been preferred in the text, and the paragraph on regelation on page 196 seems misplaced. The book is extremely well written and will certainly serve for long as an introduction to any branch of ice physics in which the reader has no specialized knowledge.

J. G. PAREN

CORRESPONDENCE

Breton Tombs

SIR,—In "Breton Origin for Tombs" (*Nature*, 228, 1019; 1970) several radiocarbon-14 dates are given in terms of absolute ages BC. I should hope your archaeological correspondent knows, and you should editorially recognize the fact, that radiocarbon-14 determinations do not produce results of this kind. Any determination is measured in years before present (BP, P being AD 1950) and has a standard deviation (for example, 5340 $\pm$ 250). The most accurate general usage of this should employ two standard deviations, that is, 4840–5840 BP or, if BC terms must be used, 2890–3890 BC.

Additionally, of course, radiocarbon years are not equivalent to calendar years, as recent tree-ring research is demonstrating. Renfrew¹ gives a useful review of this problem and its application to European prehistory in the period discussed by your correspondent, who seems to be somewhat behind the times (in publication years).

Yours faithfully,

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¹ Renfrew C. A., *World Archaeology*, 2, 199 (1970).

This letter has been shown to our correspondent, who replies:

Since the work of Stuiver and Suess in 1966 (*Radiocarbon*, 8, 543), it has been clear that radiocarbon dates are only relative dates which require calibration. Despite the implied optimism of Mr White (and of Dr Renfrew), the accurate calibration of radiocarbon dates remains a task for the future. A more recent article by Renfrew which may not yet have reached Mr White is in the *Proceedings of the Prehistoric Society* (36, 280; 1970); there is

an authoritative review by Suess in *Radiocarbon Variations and Absolute Chronology* (Proceedings of the Twelfth Nobel Symposium) (edit. by I. U. Olsson) (Wiley, 1970). When comparing dates within Europe, it remains most convenient to quote uncalibrated dates on the 5568 half-life. Whether these are expressed in years BP or BC makes not a fig of difference.

Certainly, the standard deviation of all dates should be considered ($\pm 2 \sigma$ giving about a 95% probability that the true age lies within the zone defined). The standard deviation relates only to counting error, however, and does not take into consideration other equally important potential error sources.

The essential point of my article was that the emerging pattern of radiocarbon dates for Breton passage graves sets them markedly earlier than elsewhere in Europe.

So far the Breton passage grave dates are 800 years and ten standard deviations earlier than any elsewhere. This difference and the merging pattern suggest the possibility of an independent origin of passage graves in Brittany. It remains perfectly possible, *pace* Mr White, that the earliest Neolithic settlers in England (earliest English Neolithic dates: 3390 BC $\pm$ 150 for Cissbury (BM-181) and 3505 BC $\pm$ 120 for Shippea Hill (Q-584)) were aware of Breton customs of collective burial and megalithic architecture.

Freezer Failure

SIR,—I wish to report the successive failure of no less than six ultra-low temperature electrical freezers from the same manufacturer. There are few calamities in a biological research laboratory worse than the thawing of such freezers in which are stored purified viruses, tissue culture cells, cell extracts, and purified enzymes. We have suffered considerable loss and inconvenience from these incidents, and wish to warn others of these problems.

The first of these freezers was installed in May 1969; it worked for only seven

months. One compressor developed a short circuit, causing the contents of the freezer to thaw. After three months of attempting to fix the instrument (during which time we were of course severely hampered) the manufacturers determined that it could not readily be repaired. They loaned us a temporary replacement model, and agreed to provide us with a permanent replacement. The temporary freezer functioned for two months, then developed a leak in the refrigeration system and warmed up. Fortunately, by this time our permanent replacement had arrived, and was immediately connected in its place. This new freezer worked for three months, and then failed (apparently also due to a leak in the refrigeration system). The loss of several thousand dollars' worth of tumour viruses and enzyme preparations was narrowly averted because we were able to obtain some space in an older freezer which apparently had been functioning normally for several years. However, the automatic alarm system for power failure proved to be defective. This quasi-reliable instrument thawed when the power was inadvertently disconnected by maintenance personnel.

Two other freezers were installed in May 1970 in another laboratory of this department. One worked for one month, failed for several months, and is only now working properly. The other did not work when installed and was made operative only several months later. Their futures remain in continuous doubt.

Yours faithfully,

HARVEY F. LODISH

Massachusetts Institute of Technology,
77 Massachusetts Avenue,
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Obituary

Sir Christopher Ingold

SIR CHRISTOPHER INGOLD died on December 8, 1970, at the age of seventy-seven. The classifications, concepts and terminology which he introduced provided the framework for the theory of reaction mechanisms and influenced profoundly the development of chemistry as a whole.

Christopher Kelk Ingold was born in London on October 28, 1893. He chose to study chemistry, in spite of a characteristically strong interest in physics, and took his first degree at University

College, Southampton. He went on to do research with J. F. Thorpe at Imperial College, leaving in 1918 to spend two years as a research chemist with Cassel Cyanide in Glasgow. He then returned to Imperial College in 1920 and four years later he was elected a Fellow of the Royal Society and appointed Professor of Organic Chemistry at the University of Leeds. He became Professor of Chemistry at University College, London, in 1930 and Director of Laboratories there in 1937. From 1939 to 1944 his department was in evacuation at Aberystwyth. As soon as the war ended

he began re-establishing international links and quickly built up the department again, attracting research workers from all over the world. He was appointed Professor Emeritus on his retirement in 1961.

The contributions which Ingold made to chemistry were recognized by many honours and awards. The Chemical Society awarded him the Longstaff Medal (1951) and the Faraday Medal (1962). The American Chemical Society awarded him the James Flack Norris award (1965). The Royal Society awarded him the Davy Medal (1946)



and Royal Medal (1952). He received honorary degrees not only from a number of universities in the United Kingdom but also from universities in Eire, Canada, France, Italy and Norway. He was an Honorary Foreign Member of the American Academy of Arts and Sciences, the New York Academy of Science, the Weizmann Institute of Israel, Corresponding Member of the Royal Academy of Science of Spain and Foreign Academician of the Bologna Academy of Science. He was President of the Chemical Society from 1952 to 1954 and was knighted in 1958.

Ingold's contribution to chemistry was fundamental in the sense that a transformation in thinking arose from his work. His ideas on electronic structure of molecules and the ways in which bonds are broken and formed have become embodied in concepts and terminology which run not only through research work but also through the teaching of the subject. The developments in electronic theory came first. Recognizing the importance of polarization in determining equilibrium properties, Ingold related these to molecular constituents through the ideas of inductive and mesomeric effects. Developing the theory of chemical polarizability in parallel with that of polarization, he introduced the ideas of inductomeric and electromeric effects to account for electron displacements at the demand of a reagent. This extended the electronic theory of organic molecules to cover not only equilibrium properties but also chemical reactivity, and opened up the way to theories of reaction mechanism. He recognized that in mechanisms of chemical reactions a basic classification was provided by the distinction between two types of bond fission: homolytic and heterolytic. Following up the theory of electronic displacements in molecules he introduced the distinction between electrophilic and nucleophilic reagents according to their electronic demands.

In 1930 he was joined in the development of these ideas by E. D. Hughes and the two worked in close collaboration until Hughes's death in 1963. They introduced the concept of duality of mechanism, an important step forward

because it provided a means of electronic classification that, together with earlier ones, formed the framework of a comprehensive theory of reaction mechanisms. In 1950, as Baker lecturer at Cornell University, Ingold began to bring together the developments which stemmed from these ideas and in 1953 he published his comprehensive *Structure and Mechanism in Organic Chemistry*. During his retirement he undertook the preparation of a second edition, expanding the original by some fifty per cent, bringing it up to date and completing it for publication in 1969.

From the middle 1930s onward, Ingold was also making important contributions in spectroscopy. His first work in this field was a pioneering study in the application of infrared and Raman spectra to isotopically labelled species in a detailed investigation of the benzene molecule. As a result of a second pioneering study he published an extensive set of papers on the properties of the benzene molecule in its first electronically excited state. Besides these studies, he investigated, in collaboration with G. W. King, the first excited state of acetylene, and established an important new principle, namely that electronic excitation may lead to a gross change in molecular geometry. His interests also took him into inorganic chemistry. In the early fifties he followed up ideas stemming from mechanistic studies in organic chemistry and extended these to a study of ligand replacement in metal complexes.

Ingold's knowledge of chemistry was vast and he had a remarkable insight into molecular behaviour. He combined these qualities with a powerful and critical mind which he could turn on almost any topic in chemistry. Whatever the subject of the weekly colloquia, which he rarely missed, he would unerringly go to the heart of the matter whether it concerned enzyme kinetics, spectroscopy, metal complexes or a variety of other topics. Incisively he would make his way to the centre of a problem and in a few terse sentences add illuminating comment. His breadth and depth of interest in chemistry led him naturally to build up an integrated department.

He was a courteous and kindly man, held in great affection by his students and those who worked with him. A mark of this affection was the publication in 1966 of a volume of tribute, *Studies in Chemical Structure and Reactivity*, edited by J. H. Ridd. A further mark of the affection and admiration in which he was held was the naming of the New Chemistry Building at University College after him at the official opening on September 25, 1970.

In 1924 he married Edith Hilda Usherwood who had already made important contributions to chemistry.

He was the first to acknowledge how much he owed her, particularly for her support in his scientific work and in the administration of the department. He is survived by Hilda, their son Keith and two daughters.

Announcements

University News

Professor A. V. Crewe has been appointed dean of the Division of Physical Sciences, **University of Chicago**, in succession to **Professor A. Adrian Albert**.

Professor Glenn A. Berchtold has been appointed head of the Department of Chemistry at the **Massachusetts Institute of Technology**, in succession to **Professor John Ross**.

Dr Melvin Lewis has been appointed professor of clinical pediatrics and psychiatry at **Yale University**.

ERRATUM. In *Nature* for January 15 (229, 162; 1971), Mr William van Straubenzee, Parliamentary Under-Secretary of State at the Department of Education and Science, was wrongly reported to have used a form of words advocating the abolition of the means test now applicable to student grants. His real intention was simply to explore the case for and against a gap between school and university.

ERRATUM. In the article "New Interpretation of Extragalactic Radio Sources" by M. J. Rees (*Nature*, 229, 312; 1971) the following corrections should be made: on page 313, for

$$1 \left(-\frac{v}{c} \cos \theta \right)^{-1} \text{ read}$$

$$\left(1 - \frac{v}{c} \cos \theta \right)^{-1}$$

$$\text{and for } \omega \left(\frac{\gamma}{f} \right)^{-1} \text{ read } \omega \left(\frac{\gamma}{f} \right);$$

$$\text{on page 314, for } \left(\frac{4\pi n_e e^2}{n_e} \right)^{\frac{1}{2}} \text{ read}$$

$$\left(\frac{4\pi n_e e^2}{M_e} \right)^{\frac{1}{2}}$$

$$\text{and for } 1 - \left(\frac{\omega_p}{\omega} \right)^2 \text{ read}$$

$$\left[1 - \left(\frac{\omega_p}{\omega} \right)^2 \right]^{\frac{1}{2}}$$

The requirement that waves continue to propagate outwards without dragging matter along is $\omega_p' \lesssim \omega$ not $\omega_p' \gtrsim \omega$; on page 315, line 17, for "therefore" read "thereafter". In equation (2) for $10^{4.6}$ read $10^{4.6}$, and for "infrared luminosity" read "inferred luminosity", in the second last paragraph of the same page.

British Diary

Monday, February 15

Pumps for High Velocity Fluids (6 p.m. discussion) Institution of Mechanical Engineers, at 1 Birdcage Walk, London SW1.

Science and Anti-Science (8 p.m.) Professor I. Lakatos and Dr Jerry Ravetz, British Society for Social Responsibility in Science, at the Institute of Contemporary Arts, Nash House, The Mall, London SW1.

Science in Fleet Street (5.30 p.m.) Dr A. R. Michaelis, Royal Institution, Library Circle, at 21 Albemarle Street, London W1.

The Function of the Cytochrome System and Related Redox Chain Components in Bacteria and Mitochondria (5.30 p.m.) Dr Peter Mitchell, University of London, in the Botany Theatre, University College London, Gower Street, London WC1.

The Search for More Effective Rodenticides (10.30 a.m. symposium) Society of Chemical Industry, at 14 Belgrave Square, London SW1.

Trainable Computer Programs for Circuit Design (5.30 p.m.) Mr G. C. Brown, Institution of Electrical Engineers, at Savoy Place, London WC2.

Tuesday, February 16

Cellular Abnormalities in the Brain in Chronic Liver Diseases (5.30 p.m.) Professor J. B. Cavanagh, University of London, at the Institute of Child Health, 30 Guilford Street, London WC1. (Twelfth of fifteen lectures on "The Scientific Basis of Medicine" organized by the British Postgraduate Medical Federation.)

Human Body Engineering (6.30 p.m.) Professor R. M. Kenedi, Institution of Electrical Engineers, London Graduate and Student Section, at Savoy Place, London WC2. (Silvanus P. Thomson Lecture).

Information for Senior Management (5.30 p.m.) Mr D. Firnberg, Institution of Electrical Engineers, at Savoy Place, London WC2.

Natural and Synthetic Polymers—Structure and Biodegradation (10 a.m.) Society of Chemical Industry, Agriculture Group, jointly with the Plastics and Polymer Group, at 14 Belgrave Square, London SW1.

Problems of Agricultural Migration in Ghana (5.30 p.m.) Professor W. Manshard, University of London, at Bedford College, Regent's Park, London NW1.

Quick Response Actuators in Postal Engineering—Are They the Weakest Link in Design? (6 p.m. discussion) Institution of Mechanical Engineers, at 1 Birdcage Walk, London SW1.

Surface Analysis by Auger Spectroscopy (6.30 p.m.) Mr J. C. Riviere, Society for Analytical Chemistry, jointly with the RIC, at the University, Newcastle upon Tyne.

Turkish Ceramics of the Sixteenth Century (5.45 p.m.) Mr J. Mellaart, University of London, at the Institute of Archaeology, 31–34 Gordon Square, London WC1.

Wednesday, February 17

Alfred Wegener's "Origin of Continents and Oceans", 1915 (1 p.m.) Dr P. J. Adams, Royal Institution, History of Science Discussion Group, at 21 Albemarle Street, London W1.

Electron Energy Analysis (two-day conference), Institute of Physics and the Physical Society, Electron Microscopy and Analysis Group jointly with Thin Films and Surfaces Group, at the Institution of Electrical Engineers, Savoy Place, London WC2.

Electro-medical Power Supplies (5.30 p.m.) Mr A. K. Dobbie, Institution of Electrical Engineers, at Savoy Place, London WC2.

Hazards in the Chemical Industry (10 a.m. symposium) Society of Chemical Industry, Fine Chemicals Group, at the Scientific Societies Lecture Theatre, 23 Savile Row, London W1.

Integrated Circuit Laboratories in Colleges and Universities (7 p.m.) Mr J. Butcher, Institution of Electronic and Radio Engineers, at Enfield College of Technology, Enfield.

Material Developments in Motor Vehicles (6.45 p.m.) Mr G. A. Whittaker, Manchester Metallurgical Society, in the Renold Building, UMIST, Manchester.

Mechanisms for Packaging Machinery (6 p.m. discussion) Institution of Mechanical Engineers, at 1 Birdcage Walk, London SW1.

Natural History Bookselling (6 p.m.) Mr Charles Kirke Swann, Society for the Bibliography of Natural History in the Men's Staff Common Room, University College, London, Gower Street, London WC1.

Techniques of Electrographic Reproduction (7 p.m.) Mr D. P. Maple, Oil and Colour Chemists' Association, at the Borough Polytechnic, Borough Road, London SE1.

The Thames Barrier Scheme (6 p.m.) Mr R. W. Horner, Royal Society of Arts, at John Adam Street, London WC2. (Thomas Grey Memorial Lecture).

Vibration in Buildings (2 p.m. symposium) Society of Environmental Engineers, at Salford University.

Where is University Level Education Going? (5.30 p.m.) Professor J. Brown, Institution of Electrical Engineers, at Savoy Place, London WC2.

Thursday, February 18

Art and Science: 6. Cubism and Relativity (1.30 p.m.) Mr P. D. Carpenter, University of London, at Imperial College, London SW7.

Cost Accountancy in the Seventies (2 p.m.–5 p.m. discussion) Institution of Mechanical Engineers, at 1 Birdcage Walk, London SW1.

Environmental Pollution by Agriculture (5.30 p.m.) Dr K. Mellanby, University of London, at Senate House, London WC1.

Metal Winning from Low-grade Ore by Bacterial Leaching, Mr A. W. Fletcher; **Thermodynamic Study of Tin Smelting: Iron-Tin and Iron-Tin-Oxygen Alloys**, Dr S. Y. Shiraishi and Dr H. B. Bell; **Design and Operation of a Fluo-Solid Reactor from Iron Ore Reduction**, Professor S. Y. Ezz (5 p.m.) Institution of Mining and Metallurgy, at the Geological Society, Burlington House, Piccadilly, London SW1.

Some Aspects of Liver Disease in Ugandan Africans (7.30 p.m.) Professor M. S. R. Hutt, Royal Society of Tropical Medicine and Hygiene, at Manson House, 26 Portland Place, London W1.

Some Facts About the Problems of Brain and Mind (1.20 p.m.) Professor J. Z. Young, University of London, in the Botany Theatre, University College London, Gower Street, London WC1.

The Assessment of Environmental Pollution—Current Problems in the Monitoring of Effluents for Pollution (6.30 p.m. discussion) Society for Analytical Chemistry, Automatic Methods Group, at the Leicester Lounge, 44 Glasshouse Street, London W1.

Vibrations, Waves and Vibrations (1 p.m. films) Royal Institution, at 21 Albemarle Street, London W1.

Where Paint and Ink Meet (7 p.m.) Mr R. G. Kinsman, Oil and Colour Chemists' Association, at The Beech Tree Hotel, Beaconsfield, Bucks.

Friday, February 19

512th Meeting (9.30 a.m.) Biochemical Society, at the Medical Research Council National Institute for Medical Research, The Ridgeway, Mill Hill, London NW7.

Developments in Automobile Finishing (6.30 p.m.) Mr H. L. Quick, Oil and Colour Chemists' Association, at the Chamber of Commerce House, 75 Harborne Road, Birmingham 15.

Modern Developments in Graphic Recording Devices (5.30 p.m. discussion) Institution of Electrical Engineers, at Savoy Place, London WC2.

Structural Aspects of Protein Luminescence (1 p.m.) Dr F. W. J. Teale, Royal Institution, Photochemistry Discussion Group, at 21 Albemarle Street, London W1.

The Moon and the Study of the Apollo Samples (9 p.m.) Professor S. K. Runcorn, FRS, Royal Institution, at 21 Albemarle Street, London W1.

Saturday, February 20

Zoological Aspects of the Army's Blue Nile Expedition (3.30 p.m.) Dr P. Morris, Inner London Education Authority, at the Horniman Museum, London Road, Forest Hill, London SE23.

Monday, February 22

Concorde (6.30 p.m.) Mr. B. Trubshaw, Institution of Electrical Engineers, London Graduate and Student Section, jointly with the I.Mech.E., at 1 Birdcage Walk, London SW1.

Semiconductor Nuclear Devices (2.30 p.m. colloquium) Institution of Electrical Engineers, at Savoy Place, London WC2.

Reports and Publications

not included in the monthly Books Supplement

Great Britain and Ireland

Report of the Agricultural Research Council for the year 1969/1970. Pp. vii + 93. (London: HMSO, 1970.) 13s (65p) net. [11]

Chemistry and Industry 1971 Buyers' Guide: Chemicals, Chemical Plant and Laboratory Equipment. Pp. 113. (Letchworth, Herts: The Chemical Society, 1970.) 20s. [11]

Other Countries

Institut de Physique du Globe, Faculte des Sciences de Paris. Observations Magnetiques, Dumont d'Urville (Terre Adelie) 1964. Pp. 53. Observations Magnetiques, Dumont d'Urville (Terre Adelie) 1965. Pp. 53. (Paris: Institut de Physique du Globe, Faculte des Sciences de Paris, 1970.) [1412]

Geophysical Fluid Dynamics: a Journal of the Mechanics and Energetics of Atmospheres and Oceans, Vol. 1, Nos. 1 and 2, (April 1970.) Pp. 1-272. Subscription rates: In Great Britain—Individuals who

warrant the journal is for their own personal use, per volume, postpaid 110s. Libraries, research institutions and others, per volume, postpaid 310s. Outside Great Britain—Individuals who warrant the journal is for their own personal use, per volume, postpaid \$14.50/£6. Libraries, research institutions and others, per volume, postpaid, \$41/341s 6d. (New York and London: Gordon and Breach, Science Publishers, 1970.) [1512]

Annual Report of the Museum of Applied Arts and Sciences for 1969. Pp. 23. (Sydney: Museum of Applied Arts and Sciences, 1970.) [1512]

Canada: Department of Energy, Mines and Resources. Geological Map 1195A: Gander Lake (West Half), Newfoundland. Bulletin 173: Heavy Mineral Studies in the Klondike Area, Yukon Territory. By C. F. Gleeson. Pp. 63. \$2. Paper 69-12: Geology of McKendrick Lake Map-Area, New Brunswick. By F. D. Anderson (with a contribution by George D. Hobson.) Pp. iv + 16 + map. \$1.50. Paper 70-16: A Buffering and Standard Addition Technique as an Aid in the Comprehensive Analysis of Silicates by Atomic Absorption Spectroscopy. By A. E. Foscolos and R. R. Barefoot. Pp. iv + 15. \$1.50. Paper 70-24: Reconnaissance Geology of a Part of the Precambrian Shield Northeastern Quebec and Northern Labrador, Part 2. By F. C. Taylor. Pp. v + 10 + map. \$1.50. Paper 70-43: Biogeochemical Investigations in the Lerch Lake Area, Chalk River, Ontario. By E. H. W. Hornbrook. Pp. v + 23. \$1.50. Paper 70-55: Automatic Contouring of Geophysical Data Using Bicubic Spline Interpolation. By M. T. Holyroyd and B. K. Bhattacharyya. Pp. v + 40. \$2. (Ottawa: Queen's Printer, 1970.) [1612]

Recent Developments in Microwave Multiport Networks. Edited by Akio Matsumoto. Pp. 183. (Sapporo, Japan: Research Institute of Applied Electricity, 1970.) [1712]

South Australia. 31st. Annual Report of the Council of the Institute of Medical and Veterinary Science, July 1968/June 1969. Pp. 35. (Adelaide: Institute of Medical and Veterinary Science, 1970.) [1712]

Amdel Tenth Annual Report 1970. Pp. 53. (Frewville, South Australia: Australian Mineral Development Laboratories, 1970.) [1712]

Proceedings of the Conduction Development Conference, May 27/28, 1969.

(The National Heart and Lung Institute, National Institutes of Health, Bethesda, Md.) Pp. viii + 169. (Washington, DC: Government Printing Office, 1970.) \$0.75. [1812]

US Department of the Interior: Geological Survey. Water-Supply Paper 1899-B: Water Resources of Lee County, Mississippi. By B. E. Wasson and F. H. Thomson. Pp. vi + 63. \$0.35. Water-Supply Paper 1972: The Lake Tahoe Basin, California-Nevada. By J. R. Crippen and B. R. Pavelka. Pp. v + 56. \$0.40. Water-Supply 1975: Generalization of Streamflow Characteristics from Drainage-Basin Characteristics. By D. M. Thomas and M. A. Benson. Pp. vi + 55. \$0.35. (Washington, DC: Government Printing Office, 1970.) [1812]

International Commission on Radiation Units and Measurements, Washington. ICRU Report No. 18: Specification of High Activity Gamma-Ray Sources. Pp. vi + 26. (Washington, DC: ICRU Publication, PO Box 4869, 1970.) \$2.50. [1812]

Comptes Rendus des Travaux du Laboratoire Carlsberg. Vol. 37, No. 16: Induced Potassium Uptake as a Result of Increased Medium Osmolarity in Suspensions of *Tetrahymena* Cells. By Birte Kramhoft. Pp. 343-358. 11.50 D.kr. Vol. 37, No. 17: Hydrogen-Tritium Exchange in Human Erythrocyte Carbonic Anhydrases B and C. By Richard E. Tashian. Pp. 359-370. 11.50 D.kr. (Copenhagen: Danish Science Press, Ltd., 1970.) [2312]

Peaceful Uses of Nuclear Energy. (A collection of speeches by Glenn T. Seaborg, Chairman, US Atomic Energy Commission.) Pp. iii + 156. (Oak Ridge, Tennessee: Division of Technical Information Extension, US Atomic Energy Commission, PO Box 62, 1970.) gratis. [2312]

US Department of Agriculture: Agricultural Research Service. Miscellaneous Publication No. 1015: Additions and Corrections to an Annotated List of Generic Names of the Scale Insects (Homoptera: Coccoidea). By Louise M. Russell. Pp. 23. (Washington, DC: Government Printing Office, 1970.) \$0.15. [2312]

Physical Environment of Saskatoon, Canada. Edited by E. A. Christiansen, in association with W. A. Meneley. (NRC Publication No. 11378.) Pp. ix + 68. (Saskatoon: Saskatchewan Research Council, in co-operation with The National Research Council of Canada, 1970.) [2312]

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